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views both Lennander's work and subsequent publications. He calls attention to the prominence of organic sensations in daily life,—in digestion, in intestinal pressure, in hunger, in the pains of flatulence, in esophageal pains, in sensations from the lungs and from the heart, in micturition, in defecation, in colic, and in internal irritation. Becher<sup>5</sup> later published a partially experimental study, in which he described the sensitivity of the esophagus and concluded that the stomach is insensitive. In this article he leans toward Lennander's hypothesis, in that he ascribes the feeling of vitality and other complex feelings of an organic nature to sensations from the body-wall, the pleura, the peritoneum, the diaphragm, and, probably, the esophagus. He thinks that Meumann's sensations of the stomach must have come from the body-wall. He denies sensibility to the lung tissue, the heart, and the arteries. The veins mediate pain. Meumann replies<sup>6</sup> that the reference to the body-wall will not explain the difference in quality of the different stomachic sensations, or the difference of all of them from pressure; that the extension of the wall is too slight to give intense sensations; that the feeling of satisfaction is obviously dependent upon the chemical state of the stomach; and that experiments of his own with spices have demonstrated the sensibility of the stomach to one sort of stimulation. He also insists on sensations from the lung tissue and the heart, and cites experimental work upon animals to show that the large intestine, stomach, mesentery, peritoneum, spleen, and blood-vessels are sensitive to pain. While the controversy between Meumann and Becher continued, Hertz, Cook, and Schlesinger<sup>7</sup> published in England an experimental study, which showed, among other things, the unquestionable sensibility of the esophagus to thermal and mechanical stimulation. These results agree further with those of Head, Rivers, and Sherren,<sup>8</sup> who cite the colon as an example of 'protopathic' sensibility. Becher repeated<sup>9</sup> some of the experiments of the English workers, and convinced himself of the sensibility of stomach and intestine. He thus, eventually, came into practical accord with Meumann. At the same period, Ritter<sup>10</sup> and Kast and Meltzer<sup>11</sup> explained the apparent insensibility of the internal organs, reported by Lennander, by observations and experiments which went to show that normally sensitive organs are rendered insensitive by the subcutaneous injections of cocain used in local anesthesia. Lennander's observations were based upon opera-

<sup>5</sup> Becher, E., Ueber die Sensibilität der inneren Organe, *Zeitschrift f. Psychol.*, 49, 1908, 341ff.

<sup>6</sup> Weiteres zur Frage der Sensibilität der inneren Organe und der Bedeutung der Organempfindungen, *Arch. f. d. ges. Psychol.*, 14, 1909, 279ff.

<sup>7</sup> Hertz, A. F., Cook, F., and Schlesinger, E. G., The Sensibility of the Stomach and Intestines in Man, *Jour. Physiol.*, 37, 1908, 481ff.

<sup>8</sup> Head, H., Rivers, W. H. R., and Sherren, J., The Afferent Nervous System from a New Aspect, *Brain*, 28, 1905, 99ff.

<sup>9</sup> Einige Bemerkungen über die Sensibilität der inneren Organe, *Arch. f. d. ges. Psychol.*, 15, 1909, 356ff.

<sup>10</sup> Ritter, C., Zur Frage der Sensibilität der Bauchorgane, *Centralbl. f. Chir.*, 35, 1908, 609ff.

<sup>11</sup> Kast, L. and Meltzer, S. J., On the Sensibility of the Abdominal Organs and the Influence of the Injections of Cocain upon It, *Med. Rec.*, 70, 1906, 1017ff; Die Sensibilität der Bauchorgane, *Mitteilung. a. d. Grenzgeb. d. Med. u. Chir.*, 19, 1909, 586ff.

tions in which a local anesthetic had been used. On the ground of these papers, Meumann made a final plea<sup>12</sup> for the sensitivity of most of the internal organs; and later Hertz summed up, in a book,<sup>13</sup> the work of the English investigators in evidence of widespread sensibility. In the same year, however, Mitchell<sup>14</sup> published an account of cases, by means of which he endeavored to show that, to a great extent, abdominal pain originates in the parietal peritoneum.

Besides all this work upon the general sensitivity of the viscera, a great deal has been written upon the problem of hunger and appetite. Notable publications are those of Sternberg<sup>15</sup> and Turró.<sup>16</sup> Cannon<sup>17</sup> and Cannon and Washburn<sup>18</sup> have shown the dependence of hunger upon the contractions of the stomach, and Carlson<sup>19</sup> has since confirmed their results. These investigators have also identified hunger with a form of visceral pain.

All the work upon the internal sensibility preceding 1910 has been reviewed by Neumann,<sup>20</sup> who gives a bibliography of sixty-nine titles.

The problem of the sensibility of the inner organs is threefold,—physiological, psychophysical, and psychological. The physiological problem is that of the sensibility of the various organs. In what organs do the different sensations originate? Or, more specifically: where are the nervous terminations, involved in the impulses that condition the sensations, situated? Interest in the quality of sensation is secondary to interest in its conditions. The psychophysical problem is that of the correlation of sensation and stimulus. It is answered by a determination of limens, and of the adequacy of certain stimuli for certain qualities of sensation. It leads further to the question of the localization of the sensations in various parts of the

<sup>12</sup> Weiteres zur Frage der Sensibilität der inneren Organe, und der Bedeutung der Organempfindungen, *Arch. f. d. ges. Psychol.*, 16, 1909, 228ff.

<sup>13</sup> Hertz, A. F., *The Sensibility of the Alimentary Canal*, 1911.

<sup>14</sup> Mitchell, J. F., Sensibility of the Peritoneum and Abdominal Viscera, *Jour. Am. Med. Asso.*, 57, 1911, 709ff.

<sup>15</sup> Sternberg, W., Der Hunger, *Zentralbl. f. Physiol.*, 23, 1909, 105ff; Der Appetit in der experimentellen Physiologie und in der klinischen Pathologie, *ibid.*, 23, 1909, 305ff; Physiologische Psychologie des Appetites, *Zeitschr. f. Sinnesphysiol.*, 44, 1910, 254ff; Die Physiologische Grundlage des Hungersgefühls, *ibid.*, 45, 1910, 71ff; Der Appetit in der exakten Medizin, *ibid.*, 45, 1911, 91ff; Das Appetitproblem in der Physiologie und in der Psychologie, *Zeitschr. f. Psychol.*, 59, 1911, 91ff.

<sup>16</sup> Turró, R., Die physiologische Psychologie des Hungers, *Zeitschr. f. Sinnesphysiol.*, 44, 1910, 330ff; 45, 1911, 217ff, 327ff; Psychophysiologie de la faim, *Jour. de psychol.*, 7, 1910, 289ff, 409ff; 8, 1911, 332ff, 417ff.

<sup>17</sup> Cannon, W. B., A Consideration of the Nature of Hunger, *Pop. Sci. Mo.*, 81, 1912, 291ff.

<sup>18</sup> Cannon, W. B., and Washburn, A. L., An Explanation of Hunger, *Am. Jour. Physiol.*, 29, 1912, 441ff.

<sup>19</sup> Carlson, A. J., Contributions to the Physiology of the Stomach, *ibid.*, 31, 1913, 151ff, 175ff, 212ff, 318ff; 32, 1913, 245ff, 369ff; especially, The Relation between the Contractions of the Empty Stomach and the Sensation of Hunger, 31, 1913, 175ff.

<sup>20</sup> Neumann, A., Ueber die Sensibilität der inneren Organe, *Centralbl. f. d. Grenzgeb. d. Med. u. Chir.*, 13, 1910, 401ff, 449ff, 481ff, 529ff, 573ff, 617ff, 656ff, 606ff.

body. Interest is directed equally upon quality of sensation and upon the physical values of the stimulus or the anatomical position of the parts of the body. The psychological problem is that of the quality of the sensations. It requires an exact description of a certain class of experiences as they occur. Interest in the nature of the experience, and especially in its quality, is primary.

In the actual case, no one of these problems is entirely distinct from both the others. It may be said, however, that, so far, the physiological problem has been the most vigorously attacked. Weber, Lennander, Ritter, Kast and Meltzer, all the clinical workers, and all those who experiment upon animals ask first: What organs are sensitive? Meumann and Becher are but very slightly more psychological in their point of view, although they touch at times both upon the psychophysical problem of sensitivity and upon the qualitative nature of sensation. Hertz, Cook, and Schlesinger have much the same outlook. Meumann and Becher both speak of the ability to localize the sensations; they agree that with practice the organic sensations are easily localized, and that with this practice in localization their indefinite quality disappears. They do not, however, present definite data upon the form and amount of errors of localization.

The present study attempts to deal, from the psychophysical and psychological points of view, with the problem of the sensations arising from the stimulation of the alimentary canal. The writer has endeavored, by keeping a careful record of the intensities used, to note the dependence of sensation upon intensity of stimulus. He has attended to the direction, amount, and character of the errors of localization,—recording the localization, whenever it was mentioned, and performing a special experiment for the determination of the amount of reference for one class of stimuli. He has sought to obtain a description of the psychological character of the experiences, by the taking of full introspections upon all occasions. A statement of methods and results follows.

## EXPERIMENTAL WORK

### I. *Procedure and Apparatus*

*Observers and Experimenters.* The following persons took part in the experiments:

B, the writer of this article, instructor in psychology; C, Miss J. N. Curtis, scholar in psychology; D, Mr. F. L. Dimmick, undergraduate, major subject in psychology; F, Dr. W. S. Foster, instructor in psychology; G, Miss M. E. Goudge, fellow in psychology; Ga, Mr. F. J. Gates, undergraduate, major subject in psychology; R, Mr. G. J. Rich, undergraduate, major subject in psychology.

The principal observer was B. He easily accustomed himself to the swallowing of the stimulus tubes, and his personal interest in

the experiment tended to offset the discomfort. He kept ahead of the other observers; all new series were first tried with him; and very many more were completed by him than by the others. C, D, Ga, R, and occasionally G acted as experimenters for him.

D, F, and G also acted as observers in all the work upon the esophagus and stomach. They completed the principal series with the various sorts of stimuli, although the data obtained from them were less extensive than those obtained from B. D readily learned to swallow the tubes, and in spite of a constitutionally weak stomach proved to be a very satisfactory observer. F also had little difficulty in learning to swallow the tubes, but was unable to retain them for long periods. As he never acquired the ability to talk with the tube in place, the tube had always to be removed before he gave a report. He was especially sensitive to the mechanical stimulation of the stomach and of the upper esophagus, and when the tube was in these positions frequently vomited before the stimulus could be given. G, who had a small pharynx, always swallowed the tube with great difficulty. When it was once in place, however, she found comparatively little difficulty in retaining it or in talking. R, after much patient effort, proved unable to overcome the gagging reflex and to get the tube past the pharynx, and could not be employed.

*Stimulus Tubes.* The following forms of tube were used for introduction into the esophagus or the rectum. They will be referred to hereinafter by number.

1. For thermal stimulation of the esophagus and for practice. Single tube, heavy wall, red rubber. Lumen, 3.5 mm.; outside diam., 7.0 mm. Used only in preliminary experiments and for practice in swallowing, on account of thinness of wall. A catheter was also used for practice.

2. For thermal and chemical stimulation of esophagus and stomach by injection of water or of chemical solutions. Double-walled<sup>21</sup> tube, made of  $\frac{1}{8}$  in. and  $\frac{3}{16}$  in. red rubber tubing. Lumen, 3.0 mm.; outside diam., 9.0 mm.

3. For thermal stimulation of esophagus by means of electric heating coil. Very smooth and stiff stethoscope tubing. Lumen, 5.0 mm.; outside diam., 9.0 mm. About the end of the tube is wrapped a coil of insulated copper wire. A hard rubber core is inserted within the tube at this place in order to render the coil firm. The coil consists of 3.8 meters of No. 30 double cotton-covered wire, resistance 1.5 ohms, and is 11 mm. long by 11 mm. in diam. The leads are of No. 22 double cotton-covered wire, and pass up through the lumen of the tube to heavier wires outside. The coil was soaked in rubber cement. All measurements along the tube are made from the center of the coil.

4. For thermal stimulation of the esophagus and stomach by contact with a metallic coil which conducts a current of water. Apparatus consists of a heavy outer tube of smooth red rubber, containing two small rubber tubes within it. Lumen of outer tube, 6.0 mm.; outside diam., 10.0 mm. Lumen of inner tubes, 3.0 mm.; outside diam., 5.0 mm. The inner tubes are thus forced into elliptical section within the outer tube. At the end of the tubes is a coil of brass

<sup>21</sup> The double-walled tube is necessary to prevent conduction. Cf. Becher, *Zeitschrift*, 49, 348; Hertz, *op. cit.*, 5.

tubing, 10 mm. long and 10 mm. in diam. The brass tubing has a lumen of 1.5 mm. and an outside diam. of 2.5 mm. It is coiled in  $3\frac{3}{4}$  turns. The lower end of the coil is brought up through the center, and the two small rubber tubes are tied to the two ends of the coiled brass tube. The connections were soaked in rubber cement and drawn up within the outer tube, so that the coil was flush with the outside of the outer tube.

5. For mechanical stimulation of the esophagus by inflation of bladder.<sup>22</sup> Tubing as in 1. It bears on the end a rubber bladder (condum) about 5 cm. long, which inflates to 5 by 4, 6 by 5 cm., etc. The bladder is attached to the tube by rubber cement and by wrapping with heavy thread. At the point where the bladder is wrapped, a short piece of hard rubber tubing is placed inside the soft tube, so that the wrapping can be made firm. The joint is about 1.0 cm. long. It is soaked in rubber cement. All measurements along the tube are made from the middle of the bladder.

6. For mechanical stimulation of the stomach by inflation of bladder. Exactly as 5, except that the bladder is about 15 cm. long. Measurements along the tube are made from the proximal end of the bladder.

7. For bipolar electrical stimulation of esophagus and stomach by electric shock. Stethoscope tubing as in 3. Bears at the end two tin-foil rings, each 12 mm. broad. The lower ring is 10 mm. from the end; there are 8 mm. between the rings. Two lengths of No. 24 double cotton-covered wire are carried through the lumen of the tube, and the bared ends are brought out at the positions of each ring and wrapped about the tube over a hard rubber core within. The strips of foil which constitute the rings are wrapped over the wires and held by rubber cement. (This arrangement was less satisfactory than that used later in 8, because the contacts were slightly rough and scratched the throats of the observers.) Measurements were made from a point midway between the two contacts.

8. For electrical stimulation of various points of the esophagus and stomach, without shift of the position of the tube. Tubing as in 3. It bears six pairs of contacts which are, on the average, 0, 15, 20, 25, 30, and 35 cm. respectively from the lower end of the tube (i. e., if measured to a point midway between each pair.) The contacts consist of brass rings, each of which is 6 mm. wide; inside diameter, 8.5 mm.; outside diam., 9.5 mm. Since the tube is 9 mm. in diameter, the rings indent it slightly and thus afford a fairly smooth surface. The contact at the lower end is a rounded cap, which fits over the end of the tube; it is held in place by a rivet. From each one of the twelve contacts a wire is led through the lumen of the tube. The wires are No. 22, copper, silk and cotton triple-covered. (A heavy insulation is necessary to prevent the induction shocks from jumping.) They are connected at the upper end of the tube to heavier wires leading to mercury cups. At each contact the wire pierces the wall of the tube and is soldered to the contact. Each connection is filed smooth, and the tube is filled at the point with rubber cement in order to prevent short-circuiting of the wires by water or by the acid of the stomach.

9. For thermal stimulation of the rectum by injection of water. Double-walled tube, consisting of a heavy rectal tube through which a lighter tube has been run. Lumen, 3.5 mm.; outside diam., 11.0 mm.

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<sup>22</sup> Cf. Becher, *Zeitschrift*, 49, 350; Hertz, *op. cit.*, 16.

10. For chemical stimulation of the rectum by injection of liquids. Plain, single tube of same tubing as 3.

11. For mechanical stimulation of rectum by inflation of bladder. Of same tubing as 3. Bears on the end of it a bladder, 7 by 4 cm., attached in the same manner as that of 3.

12. For bipolar electrical stimulation of rectum by induction shock. Same as 7, except that the tin-foil is less smooth and is tied on with thread.

*Arrangement of Apparatus.* A description of the apparatus employed with the tubes for the control of the various stimuli—thermal, mechanical, electrical, chemical—follows.

For the introduction of hot and cold water into the alimentary tract the heavy-walled tubes 2 and 9 were employed, in order to retard conduction by the walls of the tube and to prevent consequent direct stimulation of throat or anus. Still further to reduce the possibility of conduction through the walls, an apparatus (Fig. 1) was devised by means of which a given amount of water at a given temperature could be forced through the tube so rapidly that there was no time for the tube to cool down or to warm up materially. A 100 cc. flask is placed within a 1,000 cc. beaker, which is filled with water and kept at the temperature desired for the stimulus. The volume of the water is such that the temperature does not change rapidly, and may easily be kept within one degree by the regulation of the burner or by the occasional addition of cold water or ice. A thermometer in the large beaker shows the temperature. The water to be injected is drawn in a pipette from the beaker, measured in a graduate, and poured immediately into the flask through a funnel-tube. The opening of a pinch-cock below the funnel-tube both admits the water and also allows the expulsion of air through the parallel tube. The pinch-cock is closed after the water has been poured in, and the water is allowed to stand for about a minute in order that it may resume the temperature of the bath. It is then forced into the stomach-tube or the rectal-tube by blowing air into the flask by means of a rubber bulb. This bulb is an ordinary syringe bulb, containing valves that prevent the return of the air. With a little practice the experimenter learns the squeeze that is necessary to force a given amount of water into the body with a minimum of accompanying air.

The same apparatus was used, without the temperature bath, for the introduction of chemical solutions and solids in suspension into the alimentary canal.

Mechanical stimulation of the alimentary tract was accomplished by the inflation of a rubber bladder: tubes 5, 6, and 11. The apparatus is shown in Fig. 2. The bladder on the end of the tube is pumped up directly by a syringe, and the pressure is measured by the height of a water column. The tube for the column is 150 cm. high; it was, however, seldom necessary to go above 70 cm. The body of air in the large bottle is intended to act as a cushion, and to prevent the minor fluctuations of pressure that occur with the pumping and with the action of peristalsis upon an inflated bladder. It is necessary to have the bottle of large cross-section, in order that the rise of water in the tube will not appreciably lower the level in the bottle and thus change the zero-point. The level of the water in the system is controlled by filling from the funnel-tube, which is ordinarily shut off by a pinch-cock. The observer lies upon a couch at such a height

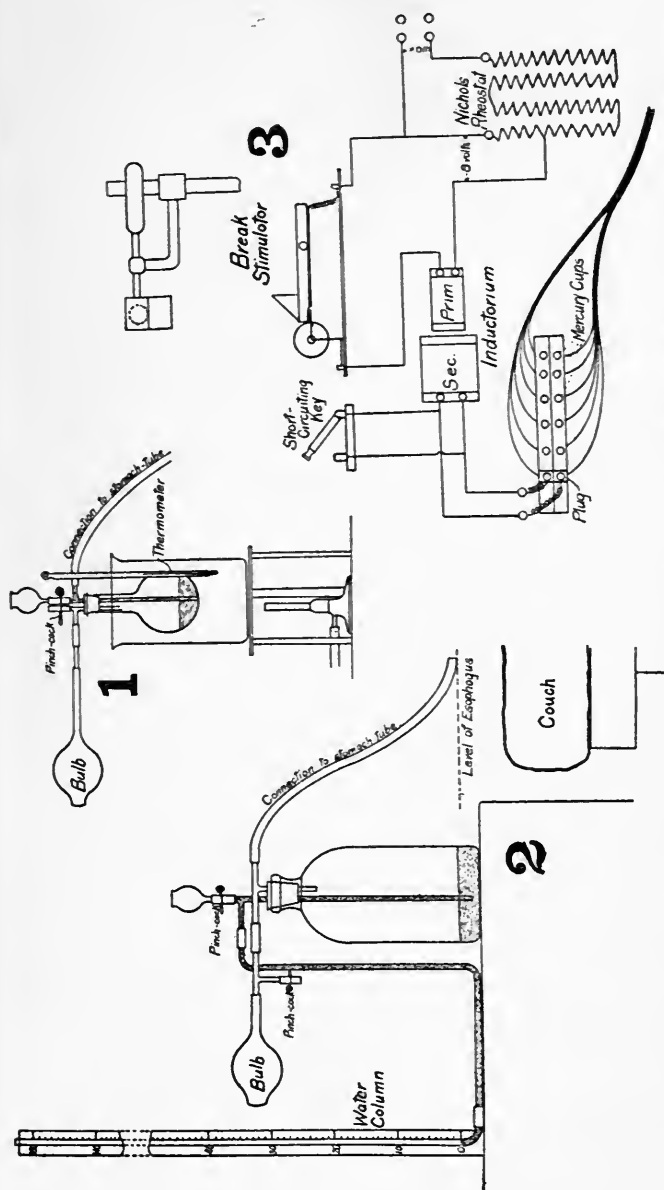


FIG. 1.—Apparatus for the introduction of a liquid at a given temperature under air pressure into the alimentary tract.

FIG. 2.—Apparatus for the inflation of a rubber bladder with air and the measurement of the pressure.

FIG. 3.—Apparatus for the bipolar electrical stimulation of the esophagus by break-shocks at 5 different points.

that the esophagus is approximately at the same level as the water in the bottle. A pinch-cock, connected with the stomach tube provides a means of release for the air after inflation.

In subsequent experiments, in which it was desired to control the rate of inflation, a bicycle pump was substituted for the rubber bulb. The pump was placed horizontally and in a convenient position and the piston rod put through a hole in a fixed metal plate. Two metal stops on the rod, one on each side of the plate, limited the length of stroke to 4 cm. The pump was worked by hand. As each stop came against the plate a metallic click resulted. The rate was controlled by the experimenter, who took care to make these clicks coincide with the clicks of a metronome. In a trial series it was found that the pressures obtained by an equal number of strokes were practically identical for rates varying from 15 to 75 per min.

For the esophagus and rectum it is probably fair to assume that the pressure measured by the water column is practically all exerted upon the tissues; that is to say, that the pressure is not reduced by the force necessary to expand the bladder alone. This assumption is approximately correct, because the bladder is confined within a tube and not allowed to expand in the normal spherical form. In the inflated bladder the only expansion is toward the end, and the internal pressure,—everywhere equal, and normal to the surface, by the law of hydrostatics,—must be transmitted almost entirely by the unexpanded side-walls of the rubber bag. Only to the extent that the esophagus or rectum stretches beyond the unexpanded diameter of the bladder is the measurement inexact. The error is probably greater in the stomach, where the bag takes more nearly the form that it would assume if it were inflated without constraint.

Fig. 3 shows the final form of the apparatus for electrical stimulation, as it was used with tube 8. The six pairs of contacts on the tube were connected with six pairs of mercury cups. The secondary of the induction coil was connected with a plug, made of a block of hard rubber bearing two pins which could be thrust simultaneously into a pair of mercury cups. In this manner the coil could be connected very readily to any pair of contacts on the tube. The induction coil is a 'standard'<sup>23</sup> coil, with a primary 13 cm. long and 10,000 turns in the secondary, manufactured by Zimmermann.<sup>24</sup> A removable core was kept within the primary throughout the trials. The scale for the secondary runs from zero to 20 cm. The coil was, however, calibrated in Kronecker units, and all values of inductive strength will be given in these units.<sup>25</sup> Only break-shocks were used; the make was eliminated by a short-circuiting key, connected with the secondary. In the early experiments a telegraph key was used to break the circuit. This procedure, however, proved so inconstant that a Titchener sound-stimulator<sup>26</sup> was substituted. In this instrument a

<sup>23</sup> The dimensions of a standard induction coil are discussed by Martin, E. G., *The Measurement of Induction Shocks*, 1912, 88f.

<sup>24</sup> No. 1901 of Catalogue 20, 1908.

<sup>25</sup> For the method of calibration, see Kronecker, H., Ueber die Ermüdung und Erholung der Muskeln, *Arbeiten aus der physiol. Anstalt z. Leipzig*, 1871, 186; Martin, *op. cit.*, 16ff. The writer is indebted to the Dept. of Physiology of the Cornell Medical College for the use of their Kronecker coil and Martin key.

<sup>26</sup> For full description and figure of this instrument see Titchener, E. B., *Experimental Psychology*, vol. ii, pt. i, 1905, 153ff.



marble is dropped from a constant height (in this experiment, 22 cm.) upon a table, which rocks slightly about a pivot, breaking a circuit between a plate on the lower surface of the table and a pin set in the circumference of a wheel that is rotated by friction with the table. The break is very quick. Constancy is secured by the fact that contact can be set in only one way and that, after the release of the marble, the operation is entirely mechanical. The current employed was taken from a Nichols rheostat connected with a 110-volt direct current line. The open-circuit voltage of the primary circuit was 8 volts.

The same apparatus was used with tubes 7 and 12, with the omission of the mercury cups. The single contacts were connected directly with a pair of binding posts in circuit with the secondary coil.

The heating coil of tube 3 was connected directly with the 8-volt connections from the Nichols frame. A variable resistance was placed in the circuit with the secondary coil, but it was found unnecessary to use it. The intensity of stimulation depended in any case upon the length of time during which the current was allowed to flow; and the increase was found to be not too great with 8 volts, whereas with less than 8 volts the initial warming-up to the limen took too long.

It should perhaps be mentioned that an unsuccessful attempt was made to use unipolar stimulation. A sponge electrode of about 100 sq. cm. was attached to the arm, and the esophagus and stomach were stimulated. The sensations in the arm were noticeable long before any sensation from the alimentary canal could be observed. The intensity might have been increased to an amount sufficient to bring out the internal sensations; but the movement and the extreme discomfort induced in the arm would have proved too great a distraction, even if it would not have raised a question regarding the exact seat of the stimulation. It would have been possible to use a larger electrode; but the desire to confine the current as far as possible to the area under investigation forbade this attempt.<sup>27</sup>

*General Procedure.* Before the experimental work began, the observers had to learn to swallow the stomach tubes. They first tried the catheter and tube 1. They were instructed to swallow on the tube, to take it as one might a bolus. To aid in the swallowing, water was sometimes allowed to trickle slowly through the tube. As we have seen, the observers differed greatly. F from the first preferred a tube large enough to be so stiff that he could thrust it down rapidly. G, on the other hand, gulped the tube down very slowly inch by inch.

When the observers were first required to talk with the tube in place, they hesitated and spoke very little. Within an hour, however, they found themselves able to talk continu-

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<sup>27</sup> Becher, however, did use unipolar stimulation with the whole arm immersed in water. He thought that, since he had proved the sensibility of the esophagus, there was but little doubt that the sensations originated there. *Zeitschrift*, 49, 352.

ously, with occasional pauses when the tube tickled the throat and started the gagging reflex. F was an exception to this rule, for he never became able to talk without vomiting, although he tried again and again. Work was thus slower with F than with the others, for it was necessary to remove the tube after each observation. The swallowing is the most uncomfortable (and sometimes painful) part of the operation. Hence in the case of F the series were not only delayed in the actual time consumed, but were also limited in the number of observations that could be taken in a single hour. For this reason fewer observations were in general taken with F than with the other observers.

Toward the end of the experiment, D and G could usually retain the tubes without serious discomfort. B had become so practised by continued work that it was not a great hardship for him to allow experimentation upon the esophagus for four hours in a single day, or to retain a tube continuously for two hours. He sometimes preferred keeping the tube in place, when going upon an errand in the laboratory, to the unpleasantness of having to swallow it again. Toward the end of the experiment he observed five hours a day for a week on the esophageal and rectal work combined. This experience was, it is true, somewhat upsetting; the explanation may lie, however, in the fact many of the stimuli were chemical.

All the experiments were performed with the observer lying supine upon a couch, which was adjusted to place him at the level of the apparatus. The stimulus tube was supported from the observer's mouth by a ring attached to a cord which ran over a pulley to a counterweight.

*Localization.* The position of the stimulus in the esophagus or stomach is recorded, throughout these experiments, as the number of centimeters from the teeth downward. The observer was instructed to keep the tube lying flat in the mouth, and not bunched up in the throat. In such a system, 15 cm. lies approximately at the junction of pharynx and esophagus; 20 cm., in the upper portion of the esophagus. For F, who is very tall (6 ft. 2 in.), this point must have been quite close to the pharynx. The gastric end of the esophagus lay between 40 and 45,—in B at about 45. This point was determined by the ease with which a bladder could be expanded within the tract, and by a radiograph (not reproduced in this paper), taken with the tube in, in which a shadow of the stomach, distended by gas, appeared. With the limits of the esophagus

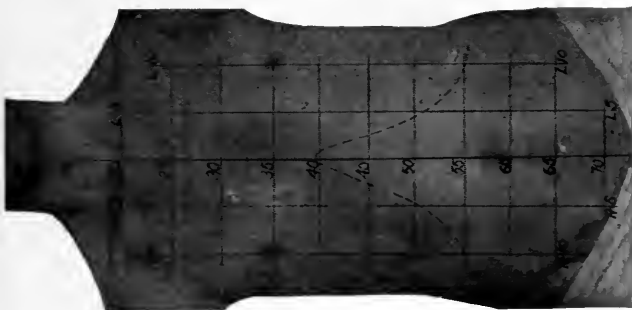


PLATE II  
Coordinates for localization in relation to superficial anatomy. Observer B. Longitudinal scale corresponds to distance from the teeth of underlying points in the esophagus.



PLATE I  
Radiograph of Observer B with esophageal tube in place. A = end of 40 cm. tube.



thus fairly defined, five points,—40, 35, 30, 25, and 20 centimeters respectively from the teeth,—were selected for investigation. They will be referred to by these numbers. The upper end of the esophagus is 20; the lower end, 40. There was no means of controlling the position of the end of the tube after it had entered the stomach. Ordinarily the setting 50 was taken for an observation upon the stomach. Occasionally 45, 55, and 60 were used.

The observers were required in all the trials to localize the sensations reported. In the cases of D and G the localization was made by reference to such definite anatomical parts as the larynx, clavicle, nipples, sternum, ribs, and umbilicus; or by indication upon a large outline diagram similar in form to the small ones shown in Figg. 4 *et seq.* The localization was recorded both verbally, and diagrammatically upon a smaller outline diagram which was placed on the record sheets by means of a rubber stamp. These general means of localization were supplemented, in the cases of B and F, by laying off the surface of the body in five-centimeter squares, as is shown in Plate II. Plate II is a photograph of B, and shows the anatomical relations. The longitudinal scale indicates the distance in cm. that a point at that level in the esophagus is from the teeth. The relation of the position of the tube, as measured from the teeth, to the surface of the body was determined for B by means of the radiograph of Plate I.<sup>28</sup> The transverse scale reads to left and right of the median axis. Thus the right nipple would be designated as 35R10, *i. e.*, 35 cm. down and 10 cm. to the right. The lines were sometimes marked off on the skin in grease pencil, the sternum, nipples and umbilicus being taken as basal points. More often however, the observer wore a 'localization map.' This 'map' consisted of a very tight-fitting undershirt, which was made to conform even more exactly to the body by elastic straps at the back and from front to back between the legs. The coördinates for localization were marked upon the shirt. Its adjustment could always be checked by reference to anatomical points.

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<sup>28</sup> The clavicle and ribs are sufficiently clear in the radiograph and could be located with sufficient distinctness in the subject to establish the local relations. Between the measurements made upon the radiograph and those made upon the subject there was an average difference of 3.8%,—a difference which was neglected.

The following anatomical relations for B in terms of the scale show the significance of the numbers:

Top of larynx.....	12.5
Bottom of larynx.....	17.5
Level of clavicle.....	20.0
Nipples. . . . .	35R10, 35L10
Lower level of sternum.....	40.0
Lower level of ribs.....	54.0
Umbilicus. . . . .	58.0
Lower level of <i>symphysis pubis</i> .....	74.0
Base of penis.....	76.0

## II. THE ESOPHAGUS

### I. Thermal Stimulation

*Sensibility.* The principal source of data upon the thermal sensibility of the esophagus is furnished by the series taken with tube 3. In these series, 35 cc. of water at different temperatures were injected under pressure into the esophagus. The method has been described above. Five positions of the tube (40, 35, 30, 25, and 20 cm.) were used with B and D, and three (40, 30, and 20 cm.) with F and G. In all these series the first observation was taken at the lowest position, and the tube was then pulled up; it was much more difficult for the observer, lying down, to have the tube pushed in than to have it pulled out. Temperatures of 0° C and 50° C were used with all the observers. With B many other temperatures were also used. In addition to these series, in which the position of the tube was varied while the temperature remained constant, a single series, in which the tube was at 30 cm. while the temperature was increased by small steps over the middle range of the temperature scale, was carried out upon each observer. The results (so far as a full introspective report, sometimes of as many as 200 words, can be reduced on its qualitative side to one or two words) are shown in Tables I, II, III, and IV for the four observers respectively. The observations recorded in these tables were taken in serial order from left to right for a single temperature, with the exception of those recorded in the column headed 30(II). This column gives the results of the series in which the temperature was increased with the tube at 30 cm. The plus sign (+) represents the successive appearance of the factors indicated.

The position of the end of the tube does not, of course, indicate the exact point of stimulation, since the water, after injection into the esophagus, continues downward. For this reason, any reports of temperature that occurred more than

TABLE I

Sensibility of esophagus to temperature. Observer B. Stimulus 25 cc. of water at given temperature introduced through tube 2. Position of end of tube recorded in cm. from teeth. Successive appearance of two factors is indicated by +.

Temp. of Water, °C.	Position of end of tube					
	40	35	30	30 (II)	25	20
0	cold	cold	cold		cold	cold
10	cold	cold	cold		cold	cold
20	cold		cold			cold
30	cold		cold			cold
35	cold		cold	cold		cold
37				cold		
38	cool		cool			cool
39				cool		
40	cool	cool	cool		cool	cool
41	+warm	+warm	+warm		+warm	+warm
43				cool		
45				+warm		
47				cool		
49				+warm		
50	cold	cold	cold	cold	cold	cold
60	+heat	+warm	+warm		+warm	+warm
70	heat	cold	cold		cold	cold
80	+heat	+heat	+heat		+heat	+heat
			heat			+pain
			+pain			

a few seconds after the beginning of the stimulation have been excluded from the tables. The water must have spread out, and have approached body-temperature, very rapidly after leaving the tube. The most intense sensation would thus probably occur somewhere in the neighborhood of the end of the tube. The generalization that the sensations reported depend upon the stimulation of the region of the esophagus at the end of the tube cannot be made with any great rigor. It is, however, supported to some extent by certain facts. (1) The esophagus can be shown to be sensitive to tempera-

TABLE II

Sensibility of esophagus to temperature. Observer D. Same as Table I.

Temp. of Water, °C.	Position of end of tube					
	40	35	30	30 (II)	25	20
0	cold	cold	cold		cold	cold
20				cold		
31				cool		
35				cool		
39				cool?		
43				warm		
47				warm		
50	warm	cool +warm	cool +warm		cool +warm	cool +warm
51				warm		
55				warm		
59				warm		
63				warm		
67				heat		
71				heat		

TABLE III

Sensibility of esophagus to temperature. Observer F. Same as Table I.

Temp. of Water, °C.	Position of end of tube			
	40	30	30 (II)	20
0	cold	cold	cold	cold
31			cold	
35			cold?	
39			nothing	
43			nothing	
47			warm	
50	warm	warm		warm
60	warm	warm		warm

ture, throughout its length, by other means (tubes 3 and 4) which will be described later, and has been shown to be thus sensitive by other investigators. The results obtained by the present method agree substantially with those obtained by the other methods. (2) The esophagus appears to be more sensitive toward the upper end than the lower. This difference



TABLE IV

Sensibility of esophagus to temperature. Observer G. Same as Table I.

Temp. of Water, °C.	Position of end of tube			
	40	30	30 (II)	20
0	cold	cold		cold
23			cold	
27			cold	
31			cold	
35			cold	
39			cool	
43			cool	
47			cool	
50	nothing	warm	+warm	warm
51			cool	
			+warm	

in sensitivity must be real, for, if the sensation were due to stimulation by water at a lower level, the upper esophagus would appear less sensitive than the lower, since, with the tube in the higher position, the water would have farther to go and would hence have assumed a more nearly neutral temperature before it had reached a sensitive region.

The following points may be noted in connection with the tables.

The esophagus appears to be sensitive throughout its length to cold at 0° C and to warmth at 50° C, though in a single instance G had no temperature sensation at 50°. Our results here are in perfect accord with those of Becher<sup>29</sup> and of Hertz, Cook and Schlesinger.<sup>30</sup>

A noteworthy fact appears in the report of B (Table I). Cool or cold sensations are reported not only for the stimuli below body-temperature, but also for all but two of the stimuli above body-temperature. In the fourteen reports of warm, every warmth is preceded by coolness or cold; in the ten reports of heat, cold precedes in every case but two. D and G occasionally, but by no means universally, find cool sensations preceding the warm. With D the cool sensations occur four times out of thirteen; with G, twice out of four. F, at the other extreme from B, never gets coolness before

<sup>29</sup> *Zeitschrift*, 49, 349f.

<sup>30</sup> *Op. cit.*, 483; Hertz, *op. cit.*, 6.

warmth. There can be no doubt about these cold sensations; they are often more clear-cut and definite than the warmth which they precede. We shall return to their consideration presently.

From inspection of the Tables it is possible to write the approximate liminal values for cold, warmth, heat, and painful heat, which are set up at a point in the middle of the esophagus:

	B	D	F	G
Cold <sup>31</sup> . . . . .	37	35	35	35
Warmth. . . . .	40	43	47	47
Heat. . . . .	60	67	..	..
Painful heat . . . . .	80	..	..	..

At the lower end of the esophagus in observer B the limen for cold is higher, and for heat lower, than is the case in the middle of the esophagus.

In order to demonstrate more conclusively that the whole esophagus is sensitive to temperature, tubes 3 and 4 were used with observer B.

The heating coil of tube 3 was allowed to become hot at the five positions usually selected in the esophagus. In all cases there appeared a sensation of warmth, followed by heat. The heat developed later into pain except at the extreme lower end of the esophagus (40 cm.). Here, too, it might have passed over into pain had the circuit been kept closed for a longer period; B feared, however, to allow the current to pass for more than fifteen seconds under the poor conditions of radiation that probably exist in the esophagus, since the coil would have become very hot and might have injured the tissue. The use of the coil demonstrates the sensibility of the esophagus easily and unequivocally. It is not adapted to quantitative determinations, as it cannot be calibrated without reference to the conditions of conduction and convection that obtain in any particular case; there is theoretically no limit to the temperature that a coil, to which electrical energy is constantly being supplied, may reach, except as a limit is established by the rate of energy-conduction away from the coil under any given set of conditions.

More satisfactory results (see Table V) were obtained with observer B from tube 4. In these trials a current of water (50 cc. altogether) was forced by means of the apparatus shown in Fig. 1 through the brass coil at the end of the stimulus tube. The esophagus proved everywhere sensitive to cold, warmth, and painful heat, with the single exception that pain only was felt for the extreme cold at the upper end of the esophagus.

This stimulus tube has the advantage over that used for the greater number of our experiments that it confines the stimulation to a small area until such time as conduction takes place through the heavy walls of the tube. It has, however, the disadvantage of affording great discomfort to the observers. In fact, after an attempt had been

<sup>31</sup> The figures for 'cold' are temperatures at which a definite, intense cold sensation is felt. 'Coolness' is seen to be reported for higher temperatures.

TABLE V

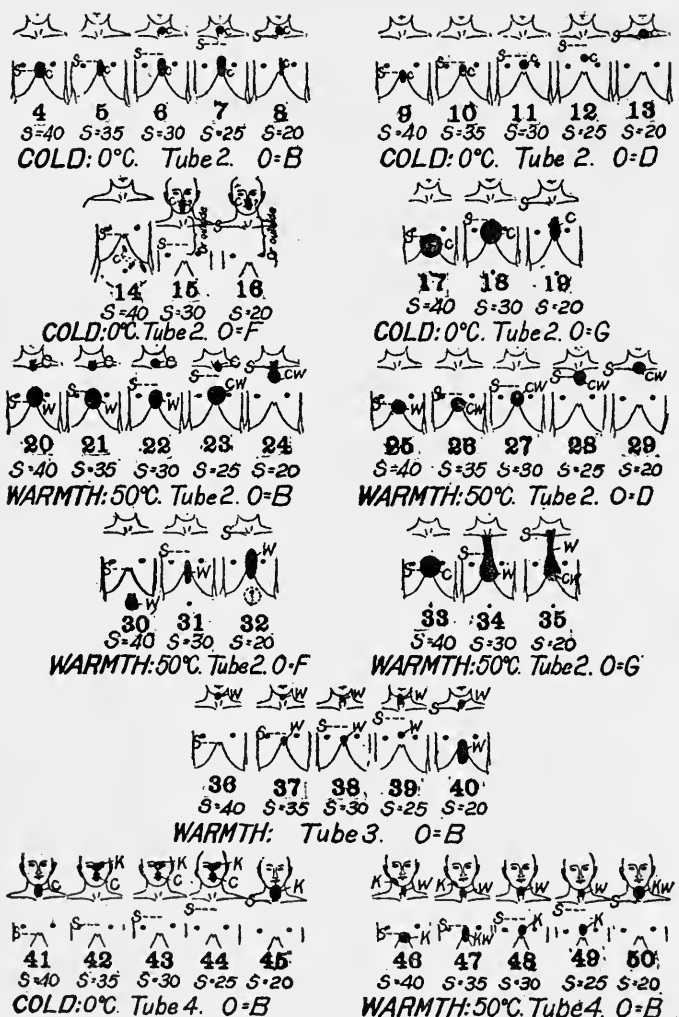
Sensibility of esophagus to temperature. Observer B. Stimulus: current of water at a given temperature passed through the brass coil of tube 4. 50 cc. of water are forced through under pressure, in each observation.

Temp. of Water, °C.	Position of stimulus coil				
	40	35	30	25	20
0	cold	cold	cold	cold	cold
50	warm	warm	warm	warm	warm
60	heat	warm	heat	heat	heat
	+pain	+pain	+pain	+pain	+pain

made to use it with F and D, the resulting soreness of the esophagus persisted for so long a time that it was considered inadvisable either to repeat the attempt on these observers or to try to use the tube with G, who has a small pharynx. The experimenter tried to construct an apparatus which should use as stimulus a loop of light rubber tubing, the ends of which were brought up through the heavy outside tube. He was unable to construct a loop which would not kink, so as to cut off the flow almost completely, even when the water was under considerable pressure. When heavier rubber was used for the inner tube, the loop changed in temperature almost as slowly as the outside tube so that the arrangement seemed scarcely preferable to tube 2. The substitution of a thin rubber sack for the loop also proved unsatisfactory, because the sack when full of water initiated peristalsis and gave rise to distracting pressure sensations.

It is tempting to call the cool and cold sensations dependent upon stimuli above body-temperature 'paradoxical.' We have seen (p. 17), however, that for observer B all stimuli from 0° C to 70° C give either coolness or cold, that there is no *Nullpunktstemperatur*. The same state of affairs occurs when the end of the tube is held in the hand; coolness precedes warmth, for the mass of the tube itself is such that the first volume of water is always cooled down to a point sufficiently below body-temperature to give rise to a sensation of coolness in the normal way. The results for tube 2 thus come into accord with those for tubes 3 and 4, with which no pure warmth sensations were obtained. The difference between observer B, who always gets coolness with warm water, and F, who never does so, may be entirely one of sensitivity. D and G are, perhaps, intermediate.

Is there then no paradoxical cold? The appearance of heat for stimuli more intense than those which give warm and with all three tubes would, on the theory that heat implies cold (von Frey), indicate a paradoxical cold. It is noteworthy that with B (Table I) the initial sensation for the higher temperatures is cold, that is to say, coolness generally precedes warmth and cold heat. It may be that the cold organs respond more rapidly than do the warm organs, as is the case when one thrusts one's hands into hot water, and that this cold is a true paradoxical cold. That it would not be felt with tubes 3 and 4 is natural enough, since these tubes present a stimulus which becomes warm very gradually.



FIGG. 4-50.—LOCALIZATION OF TEMPERATURE IN THE ESOPHAGUS

O = Observer; S = level of stimulus (cm. from teeth).

c = cold; k = pain; w = warmth.

*Localization.* The localization of the sensation of temperature for typical cases is charted in Figg. 4 to 50.

Localization of cold is shown for four observers in Figg. 4 to 19. Individual differences are apparent. B, for example,

localizes cold both in the throat and in the region of the sternum. G finds cold in the same places as the lower cold of B, but does not refer it to the throat. D tends to localize the stimuli near the sternum, except those at the upper end of the esophagus. F finds localization very difficult for the higher positions of the stimulus; he thinks that the cold may lie in the head or lower down, but he cannot make sure. "My first tendency was to connect the cold with the tactual feeling in the upper part of my throat. This did not seem right. Then I felt the cold sensations outside my body in front of me. They seem to move up and down." With tube 4 at  $0^{\circ}$  C, B refers cold universally to the throat. He does not find any sensation of temperature in the region of the stomach.

In general it may be said that the sensations, following cold stimulation are referred either toward the region of the stomach or toward the throat; that is to say, they are hardly ever localized in the upper portion of the chest.

The localization of warmth by B, D, and G is similar to that of cold. B always experiences a cold sensation preceding the warmth; the cold is felt in the neck, and the warmth at the sternum. G feels cold at the level of the sternum, but warmth frequently extends up beyond it. D localizes the preliminary cold and warmth in the same place. His localization corresponds more nearly with the position of the stimulus than that of the other observers. F refers the warmth to the abdomen or sternum, never to a higher position. He does not get sensations of cold from warm stimuli.

With the electric heating coil (tube 3), and with the brass coil for water (tube 4), B localizes the sensations in very much the same places as with the single tube (2); but in neither case does he feel cold. With the electric heating coil, the throat sensations and the sensations lower down were those of warmth. With the water coil, the throat sensations were warm, while the sensations lower down consisted of a dull pain or ache.

In general we may say that the warmth sensations tend to be localized in the region of the sternum or below, when warm water is introduced through a straight tube, although numerous exceptions occur with other methods of procedure (Figg. 36-40, 46-50). The cold aroused by warm stimuli is referred to the neck by B, to the region of the sternum by G, to the same place as the warmth by D, and is not reported by F.

*Quality of Sensation.* The reports of the observers indicate the character of the complexes aroused by thermal stimulation. The following excerpts apply for  $0^{\circ}$  C and tube 2.

B, 40 cm. "Besides usual bubbly sensations, cold developed quite definitely in long strip extending about 2 inches on either side of sternum. It lasted with some slight fluctuation to the end, by which I mean, the place where all relevant sensations stop. Considerably above this a very keen fresh cold came in,—like eating peppermint. I intended at first to localize this in my throat, but when I attended to throat it seemed as if the cold must be farther down. Then I tried to locate it with my finger, but could not decide whether it was in my throat or opposite my clavicle or considerably lower down. The thing seemed peculiarly evanescent."

B, 35 cm. "Lots of cold like that described last time. Then very intense cold extending all the way down the throat to my sternum, but although it seems continuous it seems to skip my neck. I visualize these sensations as continuous when I think of them as inside, but there seem to be about four inches of neck missing when I think of them with reference to the surface of the body."

D, 35 cm. "Cold sensations very noticeable; seem to be combined with a lump of pressure."

D, 20 cm. "Extremely cold, and sort of refreshing; quite high up in the throat."

F, 40 cm. "Moderately intense cold, which seems to be in front of bubbly sensations at body wall. Nothing is in front of my body. There was another cold inside, and a third higher up."

F, 30 cm. "My first tendency with the cold was to connect it with the tactual feeling of the tube in the upper part of my throat where I feel the tube most. This did not seem right. Then I felt the cold sensations outside my body in front of me. They seem to move up and down; and then, because I knew the length of the tube in the esophagus was not great, I tried to localize them down below the sternum; but this did not seem right either. The cold is farther in front, perhaps even outside the body."

G, 30 cm. "Auditory and pressure sensations in stomach—bubbly sensations. Cold came blended with these sensations, but did not stand out clearly at first. Afterwards, however, it became clearer. It lasts a long time; I still feel a tiny bit of cold in my stomach."

G, 20 cm. "Much the same as before. Cold not different on edges but different in midline. At first it was mixed in with stomach sensations. It stood out quite clearly after the beginning. At the end there was just a pattern of the cold sensations left."

With the water coil at  $0^{\circ}$  C and tube 4, B reports as follows:

B, 40 cm. "Very intense cold in throat and at top of pharynx. It develops and gets very intense rapidly. Coldness on the lips and tongue (probably from the conduction through the walls of the tube) come very much later. No sensations below clavicle."

B, 30 cm. "Intense cold in tiny little spots way high up above the base of the uvula."

B, 25 cm. "Cold lasts right through and becomes larger and 'achy' toward the end. I can feel this dull ache up in the region above the uvula at the same time that I feel coolness in the mouth from the tube. By a shift of attention I can also localize the cold back of the bridge of the nose, which is where I localize the intense pains which come from eating ice-cream too fast."

There is on the basis of the reports no reason to suppose that cold from the esophagus is qualitatively different from cutaneous cold. The sensations are generally referred to a large area, and the indefiniteness often ascribed to them seems to be principally a matter of difficulty of localization of this area. Either the region is not exactly placed, or its boundaries are poorly defined, or it is localized differently by different methods of judgment. B, for example, reports an indefinite sensation which he tended to localize in one place with visual imagery and in another by arm-kinesthesia. The indefiniteness is probably also a matter of intensity. Weak sensations covering a large area are apt to be called indefinite. Besides this indefiniteness, the dull pains that frequently accompany the esophageal colds are apt to give a specific qualitative coloring to the experience. Careful analysis, however, seems to separate the 'achy' cold into two components, the one of which is not unlike the cold of the skin.

The character of the sensation aroused by warm stimuli appears in the following quotations.

B, 40 cm. "Very different and quite intense cold sensations well up in region of clavicle. This cold is a little spot about two inches long and one inch wide, although the boundaries were not definitely defined. It reached maximum intensity rather quickly, though not suddenly, and died down considerably more slowly. Then a short period with no cold in it, but possibly a very weak warmth. Later the warm sensation became definite, reaching a maximum much more slowly than did the cold. At maximum intensity it seemed very slightly hot,—at least there was some qualitative change in the direction of getting more clear-cut and compact."

B, 30 cm. "Cold in throat early as before; then a less definitely localized cold lower down; followed by warmth. The warmth is quite diffuse, and not like cutaneous warmth, but as if something unpleasant were set on a background of cutaneous warmth. It is not just like heat because this something is not so sharp, definite, and clear-cut."

B, 20 cm. "This time the cold went over into warmth by way of heat. Instead of there being a low place in the temporal course between the cold and the warmth, there was a high place. I can distinguish the cold in this heat. Heat becomes less intense and then turns to intense warmth which dies out fairly rapidly."

D did not discriminate qualitatively between warmth from the skin and warmth from the esophagus.

F, 40 cm. F first describes "bubbly sensations" which are due to the injection of water through the tube and which were localized in the center of the body about the umbilicus. He finds warmth in front of the sensations and covering them.

F, 30 cm. F finds warmth as part of a complex of pressure and warmth. "The warmth is well forward in my body, very hard to localize. If you are going to get it as exactly as possible, you would say it was less than an inch below the surface of the body. No particular thickness to the warmth, which is a thin sheet."

G, 30 cm. "Weak, bubbly, followed by warm pattern. Localized warmest part just below sternum extending up into sternum. Strange to say there was also warmth from clavicle down to stomach. Wondered if I got it way up in top of throat, but decided I did not."

G, 20 cm. "Bubbly pattern, followed by weak cold in stomach; followed by and overlaid by warmth—warmer than before localized—strongest in clavicle region. Also some warmth below though this was not intense. Should say attention was on cool at first; then on definite warmth above; and then on indefinite warmth below."

It appears, then, that if any difference in quality exists between warmth from the esophagus and warmth from the skin, it is neither great nor obvious. The lower warmths, like the colds in the same region, are frequently described as indefinite or vague, but no observer calls this a qualitative difference, with the single exception (noted above) that B suggests there may be some "qualitative change in the direction of getting more clear-cut and compact."

We cannot enter here into a discussion of the possibility of a description of qualities in terms that ordinarily have an extensive connotation (*e. g.*, 'granular pressure'). As regards the validity of the qualitative distinction hinted at by B, it may be remarked that, although B found the warmth in the stomach vague and indefinite, he was able under special conditions to equate it to warmth of the skin (see p. 44). Further introspections indicate that the warmth at the lower end of the esophagus was more like heat. Of the exact analysis of heat, B has never been able to make up his mind. In it, warmth is usually clear for him. Cold is sometimes, but rarely, discernible; although cold differs from warmth in much the same way that heat does, *i. e.*, in definiteness and clear-cutness. His introspection, however, does not lead him to believe that the introduction of cold alone will change warmth to heat. When warmth becomes heat, it seems as if a 'stinging' sensation, which belongs to the family of pains, were added to it; and the existence of such a sensation is supported by the fact that it seems to occur in isolation when the glans penis is stimulated by warmth. It is possible, then, that for B stimulation of the lower esophagus by warmth tends to bring out, along with warm sensations, some sort of pain complex, which is perhaps not unlike a factor in the typical pattern for heat. This painful addition might account for the 'unpleasant warmth' described by Head as the characteristic internal warmth. In any event there seems to be no question but that the warmth from the upper esophagus is like that of the skin. All observers find them similar in character. This upper warmth passes often into a normal heat in the usual way.

*Thermal Pain.* Both cold and warm stimuli in the esophagus sometimes give rise to pain. The pain is frequently localized with the sensation of temperature. There are, however, many cases in which pain is referred to the head (see Figg. 42-47). The localization of these pains in the head is very indefinite. B nearly always finds them deep in. They



seem to alter their position for him, however, with alteration of direction of attention; *e. g.*, when visualized from the front they appear to be behind the bridge of the nose, when visualized from the mouth they appear to be up above the uvula. Sometimes, especially with warmth and nausea, an indefinite pain occurs in the jaw. It is hardly possible to place this pain more specifically than to tell which side it affects. The thermal pains are of the indefinite ache-like character of many of the organic pains. In one form they are familiar to most people, as the pains experienced in the head when eating ice-cream too rapidly.

The prevalence of 'ice-cream pains' was determined by questioning a class of forty-five students. Thirty-eight were accustomed to feel the pains when they ate ice-cream too quickly. The seven who never felt the pains did not characterize themselves as 'rapid eaters.' Those who were accustomed to them localized them variously as follows: head (2); front of head (3); head above nose, forehead over eyes, between eyes, bridge of nose, above nose, base of nose (23); temples (3); roof of mouth, soft palate (4); jaws (1); throat (1); below throat (1); intestine (1).

These pains have been explained as resulting from the stimulation of the esophageal and gastric branches of the vagus. The former ends in the esophageal plexus and lies close to the esophagus especially at the lower half. This distribution accords with the fact that the pains occur more readily upon the stimulation of the lower end than they do when the middle region of the esophagus is excited. The pharyngeal branch of the vagus may be responsible for the pains originating in the throat. The reference of the pains to the head may in some way be due to the fact that two branches of the vagus (meningeal and auricular) are distributed in this region.<sup>32</sup>

## 2. Mechanical Stimulation

*Sensibility.* The sensibility of the esophagus to mechanical stimulation was studied by inflation of the rubber bladder of tube 5. The apparatus has been described (see Fig. 2).

In Table VI are shown, in terms of cm. of water, the intensities of pressure required to produce in the first place a just noticeable sensation of pressure, and in the second place a just noticeable sensation of pain. The two values of pressure shown for B represent two separate series. The results are not exact to less than 5 cm.; because the column of water, owing to the fluctuations caused by peristalsis and the tendency of most observers to squeeze the tube involuntarily between the lips, did not remain constant.

The Table shows, not only that the esophagus is sensitive

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<sup>32</sup> Indigestion is said to be sometimes accompanied by a ringing in the ears.

to pressure and to the pain of distension, but also that its sensitivity increases toward the upper end.<sup>33</sup>

TABLE VI

Sensibility of the esophagus to pressure and to pain. Figures show the approximate intensity of pressure in cm. of water necessary to arouse a just noticeable sensation. Tube 5.

Position of Stimulus, cm.	Pressure				Pain			
	Observers				Observers			
	B	D	F	G	B	D	F	G
40	31, 21	30	30	35	40	50	40	40
35	15, 15	30		25	45	40		37
30	20, 26	25	30	25	40	45	40	32
25	12, 21	20	25	20	37	40	40	40
20	13, 25	25		5	35	40		20

The statement is ordinarily made that the limens for the sensations dependent upon the distension of the alimentary tract vary with the rate of distension. To test this assertion the rate of inflation was controlled by the use of the pump (p. 10). Five trials were taken at 30 cm. with each of five rates ranging from 15 to 180 half-strokes per min. The average liminal intensities for pressure expressed in cm. of water are as follows:

Rate (half-strokes per min.)	15	30	60	120	180
Average of 5 trials.....	33.3	28.3	23.3	21.9	19.5
Mean variation .....	4.1	4.8	7.8	4.1	1.0

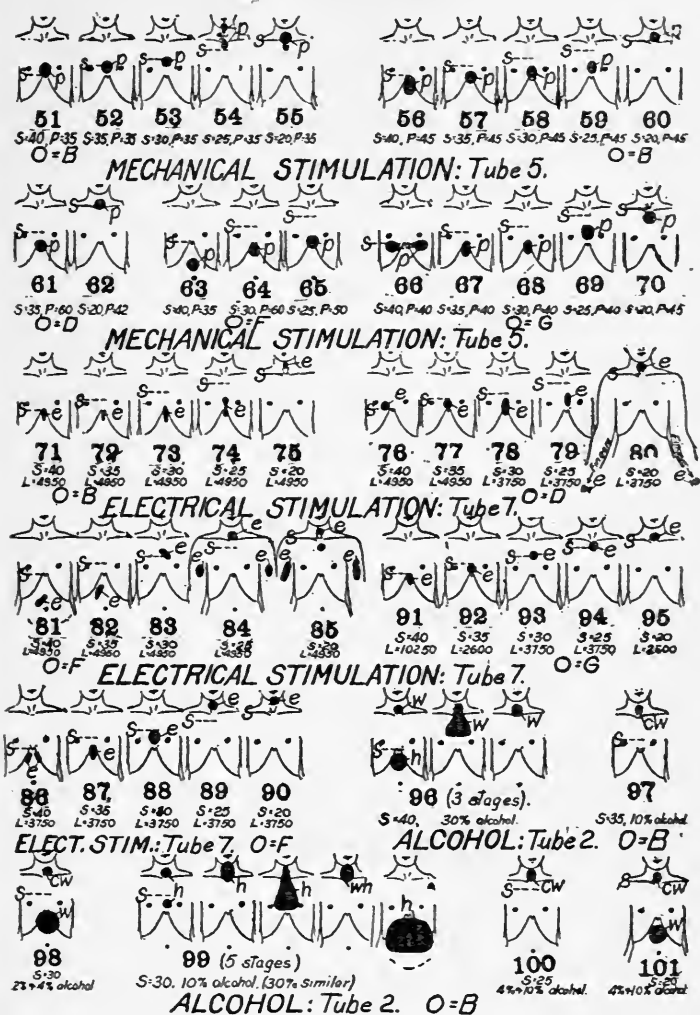
Although the M. V.'s are large, there can be no doubt but that the limen for pressure is decreased with an increase in the rate of distension.

The liminal values for pain from the inflation of the esophagus at 30 cm. are:

Rate (half-strokes per min.).....	30	60	120	180
Average of 5 trials.....	42.3	44.8	43.5	41.8
Mean variation .....	4.3	3.8	6.0	4.7

There is no evidence that the limen for pain is dependent upon the rate of inflation. This conclusion must not be drawn too

<sup>33</sup> Hertz (*op. cit.*, 16) finds a limen equivalent to 60-70 cm. of water for the sensations of 'fulness.' He denies tactile sensibility (p. 3). Becher (*Zeitschrift*, 49, 351) reports that the esophagus of his observer was sensitive to both pressure and contact.



FIGG. 51-101.—LOCALIZATION OF MECHANICAL, ELECTRICAL, AND CHEMICAL STIMULI IN THE ESOPHAGUS

O = Observer; L = intensity of electric shock (Kronecker units); P = maximum intensity of pressure (cm. of water); S = level of stimulus (cm. from teeth).

c = cold; e = electric shock; h = heat; p = pressure; w = warmth.

rigorously, however. At these high pressures peristalsis is induced so vigorously<sup>34</sup> that the water column in the manometer varies rapidly, making an exact reading difficult or impossible. The peristalsis, moreover, draws upon the tube and the resulting sensations are very distracting. Further, the sensational complex is so complicated that the appearance of an additional factor in the form of a just noticeable pain is inherently difficult to determine introspectively.

*Localization.* The localization of these complexes of pressure and pain is shown in Figg. 51-70. The same phenomenon recurs that was observed with temperature, *i. e.*, a tendency to refer sensations in both directions away from the upper portion of the chest. A difference appears, however, in the fact that none of the pressure sensations are referred to points above the actual place of stimulation. In B and D, the two upper points are sometimes localized correctly. The lower points, although varying in height with the height of the stimulus, are placed very much too low. All the localizations of G and F are below the stimulus. F makes almost as great errors for the lowest point of the esophagus as for the upper points. Some of his localizations (not shown on the figures) were as low as the umbilicus.

*Quality of Sensation.* The character of the experience resulting from mechanical stimulation is shown by the following extracts from the reports of the observers.

B, 40 cm. Max. press. = 35. "Pressure first felt as below clavicle, 6 to 7 cm., or as up in top of back of throat,—the usual confusion. As pressure got more intense, it got more unpleasant. Suddenly I realized that this feeling was exactly the thing that gives warning when one is about to vomit. It is in nausea, but so far as I can tell is purely pressure."

B, 40 cm. Max. press. = 45. "At first pressure, very much like a dull pressure, *i. e.* subcutaneous pressure; very soon, however, it becomes dull and 'achy.' The ache is not definitely pain. It is the peculiar characteristic of these internal pressures, and seems more to color them than to be a separate component. The complex is something like the ache of muscular pressure, but more definite than I have ever felt in muscular pressure, unless it be in fatigue."

D, 40 cm. Max. press. = 35. "At first pressure; then felt sort of bloaty. Fullness contrasts with my feeling afterwards when the air is released."

D, 35 cm. Max. press. = 60. "Felt definite pressure below sternum and a tendency for the tube to pull down. Pressure then got more intense and 'achy;' resembled something that swallows hard."

F, 40 cm. Max. press. = 35. "Very pressury sensations in a small

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<sup>34</sup> Cf. Hertz, *op. cit.*, 17.

lump, like a kind of a ball of muscular pressure. Intensity rises gradually; the sensations did not flash out."

F, 30 cm. Max. press. = 45. "Diffuse. This time the sensations were not so pressury; quite a good deal more intense; later the sensations went way up to the head and around and up to the nose." Q. "Pressure sensations?" A. "No. Much livelier than muscular pressure; more the quality that muscular pressure has when it gets quite sore and unpleasant; thinner and finer in diffusion than muscular."

F, 30 cm. Max. press. = 20. "Ball of muscular pressure below end of sternum stayed a little while and then disappeared."

G, 40 cm. Max. press. = 40. "At first pressure with slight coloring of pain localized in the ribs. Very funny. Both sides, sometimes front. A little later, the pain constituent becomes prominent, although not so very prominent."

G, 30 cm. Max. press. = 40. "At first pressure at sternum at middle line. Then pressure became more intense and a little crampy. Later it became very crampy and unpleasant."

G states after the series that "all these pressures are dull, heavy pressures like muscular pressure, not cutaneous in quality."

The introspections indicate that the esophagus mediates a pressure, which is like a muscular pressure, if not identical with it. In order to make the comparison more definite, the experimenter tried giving the esophageal complex and a complex produced by deep pressure on the arm simultaneously and in rapid succession. The observer swallowed the tube to 30 cm. Then the skin of the upper forearm was anesthetised by spraying with ether. A pressure, at the anesthetised portion, was produced by a Cattell algometer. Two sorts of trials were made; one, in which the intensity of the pressure on the arm was sufficient to bring out deep pain, and in which the amount of inflation of the bladder was sufficient to elicit the ache-like "crampy sensations;" and another, in which the intensities were weak enough to produce only pressure sensations in both esophagus and arm.

D, in comparing esophageal pressure of 60 cm. of water with an arm pressure of 3.5 kg., thought that the pressure sensations were identical in quality, although the one on the arm was more definite and less diffuse. The pains, he found, were both different from cutaneous pains. He was not sure whether they were alike. The pain from the arm was less like an 'achy' pain, was harder. His introspection upon the weaker intensities (30 cm. of water and 2 kg.) bears out his earlier report that the pressure sensations are alike in quality, although differing in extensive and intensive patterns.

F, after comparing 60 cm. of water in the esophagus with 3.8 kg., reports: "I can not find the slightest difference so far as the quality is concerned. The pain strains were of slightly greater intensity in the esophagus and were of wider extent."

After observing the weaker intensities (32 cm. of water and 1.8 kg.), F reported: "I do not know what I mean by thick and dull, but

certainly the arm sensation was much thicker and duller than the sensation in the esophagus, had much more body to it, was heavier. It seems to me as if the pressure in the esophagus went through a series of stages, from the weak pressure right up through the straining, deep-pain thing, in more or less of a straight line, although you can tell when the pain comes; whereas in the arm you can pick out the place where the new quality appears."

G, after being practised in discrimination of kinesthetic sensations in the arm, is asked to compare 50 cm. of water in the esophagus with 2.5 kg. She reports: "Muscular pressure in chest with tendinous strain, forming a crampy complex. In the arm there was first muscular pressure. Then pain gradually began to be fused as it got stronger. The complex in chest was crampy in character."

With the weaker intensities (25 cm. of water and 1.2 kg.), G says: "In my arm I got a sort of muscular pressure; localized within; thick, not areal. In the chest there was a scrappy lump before blowing up. This became less, while muscular pressure gradually became clearer. At first it seemed like the pressure from the arm, and then became more distinctly muscular and, I think, tendinous. When the tendinous sensation came, it had a very slightly crampy character, a sort of strain."

It appears that D and F identify the pressure components of the esophageal complex unequivocally with muscular pressure in the arm. G observes that the esophageal pressure is more crampy in character, and is inclined to suppose that it must be of the tendinous order of pressures. We have already seen that F had difficulty in deciding whether the internal pressure was painful. His difficulty arose from the fact that the muscular pressure seemed to go over without sensible break into a painful pressure. It may be that G has the same difficulty. It is more probable, however, that she failed to experience the muscular pressure in isolation because (as she insists in her reports) the painful sensations from the end of the tube were never absent. G does not herself call the arm sensations muscular, but designates them as "deep pressure," in distinction from "muscular pressure," which approaches tendinous strain and which is found in the esophagus. B did not make the comparison between arm and esophageal pressures, but was convinced throughout that the pressure from the esophagus was of the same general order as muscular pressure.

All the observers agree that there is no qualitative difference between the sensations of pain from the esophagus and those from the arm, although they find very different extensive and intensive patterns. The pain in the arm is sharply defined and intense throughout; it is also often more definitely localized. The pain in the esophagus is much less definite in extent and less uniform in intensity.

3. *Electrical Stimulation.*

*Sensibility.* In these series, bipolar electrical stimulation (tube 7) was used in the manner and under the conditions described above (p. 10). All the observers were sensitive to electrical shock throughout the length of the esophagus. Four columns of Table VII show the approximate minimal intensities of break-shock required to produce a sensation. It will be seen that the sensitivity of the esophagus increases from the lower end up. The very small value for G at 40 cm. is possibly due to projection of the lower pole below the lower end of the esophagus. G's esophagus probably does not in any event extend below 41 cm. from the teeth; and since the measurements were taken to a point midway between the two poles, the lower pole would have been more than 40 cm. down.

TABLE VII

Sensibility of the esophagus to electrical stimulation. Figures show the intensities of shock in Kronecker units (8 volts primary circuit) which brought out a just noticeable sensation. The last column gives a set of equivalent intensities for observer B.

Position of Stimulus	Intensity of induction shock in Kronecker units				
	For just noticeable sensation				For equivalent intensities of sensation
	Observers				Observer
	B	D	F	G	B
40	3750	3750	3750	10250	3400
35	2600	3750	3750	2600	2800
30	2600	3750	3750	2600	2350
25	1500	1500	700	1500	2350
20	700	1500	700	700	1300

In a subsequent series for localization (p. 36), the writer found it necessary to determine the equivalent intensity of electrical shock for the different parts of the esophagus. This determination was made with the apparatus for localization without knowledge (tube 8; see p. 7). A setting of the secondary coil at 2,800 units was found to give a shock which was fairly intense but not uncomfortable. Accordingly, 2,800

at 35 cm. was selected as a standard. Series were taken in which the other points were compared with this standard. In every case, the intensity of the point compared was varied from weaker to stronger, and also from stronger to weaker. The order of variable and standard was reversed at random, so that the observer was not prejudiced by knowledge of conditions. The variations were made by 2 mm. steps upon the scale of the inductorium (equivalent to about 100 Kroecker units). In practically all cases, the points of equality in the ascending and descending series agreed, and were selected as the stimulus value corresponding to equivalent intensities. They are given in the last column of Table VII. They indicate further that the sensibility of the esophagus increases toward the upper end; and that the increase is rapid at both ends and very small in the middle region.

The large number of units required to produce a just noticeable sensation indicates the high absolute limen of sensitivity of the esophagus. We have noted above (p. 11), in discussing the impossibility of using unipolar stimulation, that the periphery, even with a relatively larger electrode, is very much more sensitive than is the esophagus. This statement applies only to the lower portions of the esophagus. The upper portions are probably as sensitive as the skin, if not more so. A just noticeable sensation upon the skin occurs when the coil is set at 1,500, and the fingers are moistened and held firmly on the terminals.

*Localization.* The localization of these sensations is shown in Figg. 71-95. With B and D the reference seems to be much the same as for pressure and temperature (Figg. 9-13). The error of localization is such that the indicated points do not lie upon the upper chest, but upon places above or below. F, with weak stimuli, gives a similar result. G shows a remarkably small error, even indicating two positions on the chest correctly.

With intense stimuli in the upper region of the esophagus F notices sensations in the upper arm. D also refers a moderately intense stimulus at the same place to the fingers. With intense stimuli in this region B regularly feels shocks in the fingers and in portions of the arm (most frequently in the upper arm, less frequently in the shoulders).

It may be objected that the spread of the current, and the stimulation by an electrical shock of nerve trunks at points other than their terminations, render this method ill-adapted



for a study of localization; and it may be added that some of the shocks are obviously localized far from the seat of stimulation. But the shocks in the arm are so different in character from the pressure-like sensation elicited by a weak shock in the esophagus that confusion is unlikely to occur so long as the stimuli are weak.

*Quality of Sensation.* The following extracts from the reports show the quality of sensation brought out by a weak electrical shock. Intense shocks gave complexes quite as difficult to describe as those aroused by intense shocks on the surface of the body.

B, 40 cm. Coil = 4950. "A different sort of catchy pressure, dull and 'achy.' I think that I have exactly the same sensation in swallowing something too large."

B, 20 cm. Coil = 4950. "Pressure faint and tiny; finer, narrower, brighter, and more cutaneously alive than before; more definite and also more like other peripheral experiences."

D, 40 cm. Coil = 4950. "Pressure, somehow mingled with pain. A catch, rather unpleasant."

D, 30 cm. Coil = 3750. "Quite a bit like swallowing something hard; seemed to extend up and down for an inch and a half."

D, 20 cm. Coil = 3750. "Different from all the rest; more like an electrical shock." Q. "Can you say anything about the quality?" A. "Like a very hard pressure; something like being up against a corner; a little bit of numbness."

F, 40 cm. Coil = 3750. "Sensation of very weak absolute intensity. Fine lines of sensation, rather than an area of quality; like the jerky muscular sensation that you get when a muscle is suddenly contracted electrically or otherwise: something very close to strain in it; because it is strain. I think there is a background of sensations spread over the whole area, but it is very weak indeed and very diffuse. I can hardly be sure it is there,—like the ticking of a watch far away. The quality is dull. It seems as if to describe it as a little 'organic stir' was right, especially as this is the sort of thing one has when one is stirred or affected."

F, 25 cm. Coil = 2600. "Sensation is more of the straining kind than anything else in the world." F again insists on the affective character of these sensations and describes them as "excitement" or a "stir up."

G, 40 cm. Coil = 10250. "Muscular sensation over very small area; intensity rather weak; meant a twitch."

G, 25 cm. Coil = 3750. "Quality is little different. Pain component was present; darting. Whole thing seems to have finer texture, so to speak. Distinctly a little throb, with a lancing, darting pain." G implies later that muscular sensations are involved in the throb.

It is clear that electrical stimulation does not bring out any new qualities of sensation. On the other hand, the results support our conclusion that the esophagus is sensible to

pressure throughout its length; for these complexes are all formed of pressure and pain.<sup>35</sup>

#### 4. Chemical Stimulation

All the experiments with chemicals were performed solely on observer B.

*Alcohol.* Five cc. were administered in each trial with tube 2.

The esophagus appears to be sensitive to stimulation by alcohol throughout its length. Ten *per cent.* alcohol brought out sensations at 35, 25, and 20 cm., and heat at 30 cm. Cold was also felt in the throat at all these places and also at 40 cm. Warmth, however, did not appear at 40 cm. Thirty *per cent.* alcohol gives warmth combined with stinging sensations at 40 cm. Slight warmth was felt in the upper parts of the esophagus for a 4% solution; and even a 2% solution gave very slight warmth at 30 cm.

The localization of typical sensations for different intensities of alcohol is shown in Figg. 96-101. Fig. 99 shows the tendency for the sensation to spread. The final stage in Fig. 99 doubtless occurs when the alcohol reaches the stomach.

The character of the experience dependent upon stimulation by alcohol may be shown as follows.

B, 30 cm. 30% alcohol. "At first coolness. Then warmth develops and gets very slightly hot. It spreads as an intense warmth with a heat component. It is well below the surface of the body and uniform over the whole area. It shrinks from the chest up toward the throat until there is just a large general warmth, left in the throat."

B, 30 cm. 10% alcohol. "Decided cold in throat at first. Then a momentary heat in throat together with a little catch of heat opposite the nipples. Then nothing; and then a burning glow in throat, *i. e.* a diffuse, indefinitely localized heat. The heat becomes intense, so that the whole upper part of the middle of the body seems to glow with it. It seems then to shrink from below up and is soon confined to the throat only. Next it gets gradually weaker, and disappears at the time that a very strong, stinging heat develops over a large area in the stomach region. This heat dies down quickly until there remain only weak intermittent glows and stings."

*Hydrochloric Acid.* Five cc. of 5% HCl were introduced through tube 2 into the esophagus at 40, 30, and 20 cm. from the teeth. The esophagus responded in all cases with an ache-like or a stinging pain, generally very faint. These pains were localized either in the throat or somewhere below the level of the nipples. Portions of the reports follow.

B, 40 cm. 5% HCl. "Very dull ache of the tooth-ache kind, back

<sup>35</sup> The descriptions are not unlike those of Becher's Herr W., although our observers do not report a qualitative difference between the two ends of the esophagus. Becher says: "In der ganzen Speiseröhre ergeben sich sehr deutliche Sensationen. Doch ist der Eindruck nicht überall der gleiche. . . . Im oberen Teile der Speiseröhre hat Herr W. ganz deutlich den Eindruck des Elektrisierens, also jenes charakteristischen Schwirrens. . . . Im unteren Teile bleibt nur noch eine schwach brennende oder ätzende Wirkung, die Herr W. mit der eines scharfen Salzes, eines bissenden Geschmacksreizes vergleicht." (*Zeitschrift*, 49, 352.)

of the nose; then a stinging ache, *i. e.*, a raw feeling, in the throat. This ache goes quickly; then there is a much stronger ache, which is also 'stingy,'—a sort of burn without warmth. It becomes quite intense and dies away very slowly, fusing with general ache in stomach region. This latter ache gets more intense and presently without qualitative change turns into hunger."

B, 30 cm. 5% HCl. "Intense cold in throat. Then a stinging feeling above stomach, which was more like pain than a dull ache. Shortly afterward a dull ache appeared in the same place and also lower down, and turned presently into hunger pains."

B, 20 cm. 5% HCl. "Cold at first, then stinging pain from nipples to sternum. Then intense aching pains in stomach, rather sharp, bright, and diffuse. The ache later gets quite acute, like some of the pains in the call to defecation. Still later there is nausea, which involves both this ache and certain muscular sensations."

*Mustard.* In this series, 25 cc. of a 20% mixture of English mustard and water were injected into the esophagus. Aside from sensations of coolness and warmth, localized in the throat, very faint, and of very short duration, the esophagus does not seem to be sensitive to mustard; although characteristic experiences involving nausea, general aches, and specific burning aches in the throat, occur approximately at the time that the mustard reaches the stomach.

*Pepper.* In this series 25 cc. of powdered black pepper in suspension in water (1:100) were used. In each case a coolness is felt at once in the throat; later stinging sensations are also felt in the throat. The stinging sensations are the more delayed, the farther up in the esophagus the pepper is applied. Consequently we may assume that they come from stimulation of the stomach.

*Peppermint.* A 2% dilution of oil of peppermint in olive oil was used. Five cc. were injected. The introspections indicate that the esophagus does not respond to oil of peppermint. The warm sensations occur after the oil has had time to reach the stomach. Olive oil alone brought out no response.

(Numbers in brackets indicate the number of seconds elapsed after the administration of the stimulus.)

B, 40 cm. 2% oil of peppermint. "[16] Cold around the sternum; then a slight burning sensation in the throat, which soon disappears. [33] Cold around sternum gradually extends on up with slight ache."

B, 30 cm. 2% oil of peppermint. "Cold in throat at once. [23] General stinging cold; turns gradually into a warm glow all along median line. [49] Glow gets very intense, a diffuse heat. No more cold. The glow is intense all the way from top of larynx to just below sternum. It dies away in throat last, and as it dies away a cold reappears."

B, 20 cm. 5% oil of peppermint. "At first, very cold in throat. [35] Faint ache in stomach. [37] Burning heat in throat begins, and gets quite intense, but does not spread below throat; lasts a long time. [230] Heat is now dying out. Some cold has returned."

## 5. Localization

It has already been shown that esophageal sensations are variously localized by different individuals and, under different conditions, by the same individual. In general, the

reference is downward for most of the sensations from the lower two-thirds of the esophagus, and the points of reference tend to group themselves, so that the error of localization is greater for higher points of stimulation. The upper third of the esophagus, however, is apt to give sensations localized above the point of arousal. Stimulation of the lower end of the tube often results in sensations in the throat, concomitant with those higher up, and either of the same or of a different quality. In the series that we are about to describe, an attempt was made to measure the errors of localization. In all cases so far mentioned, the data have been obtained by the use of a stimulus tube with but a single place upon it for stimulation; the tube had to be moved up and down the esophagus. It was impossible to keep the observer in ignorance of the position of the tube. Even had he obtained no cues from the serial method followed, or from the length of tube outside of the mouth, he might still have obtained them from the ache-like sensations aroused by the end of the tube. For these reasons tube 8 was so constructed that it could be left in the esophagus and stomach, and electric shocks could be given at various points without altering its position.

It first appeared that the esophagus was unequally sensitive at different portions of its length; so that, when equal intensities of shock were used, an intensive cue to the point of excitation was afforded. B, the only observer, knew that the lower end of the esophagus was in general less sensitive than the upper end, and was therefore inclined to look for the weaker shock in the region of the sternum. For this reason, equivalent intensities for the different parts of the esophagus were determined in the manner described (pp. 31f.). We had planned to include a point of stimulation in the stomach, and had therefore attached the sixth pair of terminals at the end of the tube. There was, however, so marked a difference in the contact made in the stomach at various times (due, probably, both to the chance position of the end of the tube and to the nature and amount of the stomachic contents), that it was impossible to get the same intensity of sensation from the same intensity of current. Since these high intensive differences were further complicated by variation in quality, we decided to exclude the stomach from this series of trials.

Twenty-five series were arranged, in each of which each of the five points in the esophagus occurred once in haphazard order, determined by drawing numbers from a box. The observer lay supine, blindfolded, and with arms stretched at his sides. Localization was made by bringing the right hand up to the body after the shock had been felt, and indicating the point with the forefinger. The observer was allowed to move the hand after he had touched the body. In a preliminary series it was found that there were a certain number of very indefinite sensations, which the observer experienced great difficulty in localizing. Hence, in the regular series, he was allowed to ask for the repetition of the stimulus when he felt very

uncertain as to its position. Sensations that seemed to cover an area, or to stretch out in length, were localized at the point which appeared to the observer as the middle.

TABLE VIII

Localization of electrical stimulation of five different points in the esophagus. All figures, including the column headings, which indicate the point of stimulation, represent cm. along the alimentary tract from the teeth. The values are rearranged to show the distribution.

Point Stimulated	40	35	30	25	20
	48.5	46.	49.	45.	24.
	48.	43.5	42.	38.	16.
	48.	43.5	41.	37.	15.5
	48.	42.	41.	37.	15.5
	47.	42.	40.5	36.	15.
	47.	42.	40.	36.	15.
	46.	42.	39.5	36.	15.
	46.	41.	39.5	35.	14.5
	45.5	41.	39.	34.5	14.5
	44.5	40.	38.5	33.5	14.5
	43.	40.	37.5	33.	14.5
	42.5	40.	37.5	33.	14.
	42.5	39.	37.	32.5	14.
	42.5	39.	37.	32.	14.
	41.	37.	37.	32.	14.
	41.	36.5	37.	26.	14.
	40.	36.5	36.5	24.	13.5
	40.	36.	36.5	22.	13.5
	39.5	35.	36.	18.5	13.5
	38.5	34.5	36.	17.	13.5
	38.5	33.	36.	17.	13.
	38.	32.5	34.5	16.5	13.
	36.5	32.	34.5	15.5	12.5
	36.	32.	34.	15.	12.
	36.	27.	33.	15.	12.
Average.....	42.52	38.12	38.00	30.68	14.42
Av. error.....	2.52	3.12	8.00	5.68	5.68
M. V.....	3.38	3.77	2.40	7.60	1.27
Av. error.....				10.33	6.35
M. V.....				2.32	3.19

The localizations are shown in Table VIII, in which the values are rearranged so that their distribution is evident. The M. V. for 25 cm. is very large. Its size is due to the fact that the points fall into two groups, the one considerably above the point of stimulation, the other very much below. If the points are arbitrarily separated into these two groups,

so that all above 32 are in the one and all below 26 in the other (there are none between 26 and 32), the M. V.'s are greatly reduced. The figures for this separation are shown below the last line in the column for 25 cm.

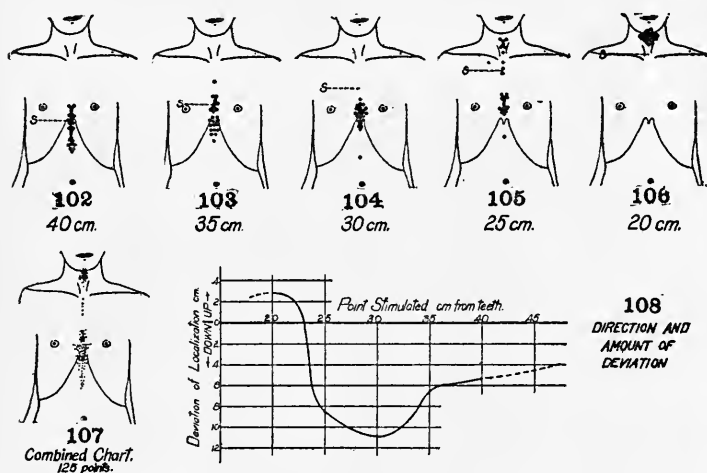


FIG. 102-108.—LOCALIZATION OF ELECTRICAL STIMULI IN THE ESOPHAGUS

102-106. Points of reference for 25 stimuli at each of 5 positions, viz., 40, 35, 30, 25, and 20 cm. from the teeth. 107. Combination of 5 preceding diagrams. Note reference away from upper chest. Points show longitudinal, not transverse, reference. S = level of stimulus. 108. Curve, showing amount and direction of reference.

Diagrams for each of the five points of stimulation are shown in Fig. 102-106. The position of the points in these figures is correct longitudinally, but not transversely, since almost all were localized on the median line (only one point was localized more than 1 cm. to one side). Fig. 107 is a combination of the five figures preceding, and shows the longitudinal position of all the 125 points. Fig. 108 shows these amounts of deviation (*i. e.*, the average errors) platted in the form of a curve. The curve is steep below 25 because there are few localizations in this region. The dotted lines indicate a probable extension of the curve.

The result is what we had expected from the rough localizations of the preceding experiments. Reference is away from the upper portion of the chest. This fact was recognized, at least by B and F, in their introspections. B constantly

complained that inside himself he "had no neck;" that is to say, he felt sensations in his throat and in the region of the sternum, and believed them to be part of the same complex, although they did not seem to be localized in the intermediate region. B explained the continuity by saying that the sensations were continuous when visualized from within, but they were discontinuous when visualized as underlying the various parts of the body. The difference was very striking: when he visualized from the outside, there was a constant fluctuation of the visual image (more often an alternation than a co-existence); but, when he visualized from within, not only was there but a single sensation to be found, but there seemed to be nothing else to which he could attend. Apparently the spatial pattern within the chest does not correspond with that of the outside.

On the basis of these results we cannot agree with the statements of Becher. Concerning cold and warmth sensations in the esophagus he writes: "Die Lokalisation— in der Höhen-dimension und nach einer obigen Bemerkung über die Feststellung der unteren Linksabweichung auch in der Breiten-dimension—ist innerhalb gewisser Grenzen zuverlässig."<sup>36</sup> "Herr W. empfindet also zweifellos in der ganzen Speiseröhre Druck- und Berührungsreize. Diese werden ziemlich gut lokalisiert."<sup>37</sup> For electrical stimuli, "das Lokalisieren ist ziemlich gut, wie bei den Druckreizen."<sup>38</sup> Hertz is even more positive and states "that the whole esophagus is sensitive to thermal stimuli, and that the power of localization is extremely accurate, particularly in the case of heat."<sup>39</sup>

The tendency to refer the sensations which originate in the esophagus in the region of chest to the soft parts of the trunk may throw some light upon the means by which ability to localize is acquired. Our experiment requires the observer to localize sensations from the esophagus upon the surface of the body. The only parts of the body-wall on which a stimulus, affecting the surface of the body, would also affect the esophagus, are the soft parts below the sternum and above the clavicle. These parts may thus have become associated in experience to the corresponding parts of the esophagus. The probability that stimuli below the sternum would affect the esophagus farther up under the hard chest-wall than the stimuli at the neck would affect it downward and underneath may explain the facts (1) that sensations underneath the chest-wall are referred a greater distance downward toward the sternum than they are upward toward the neck, and (2) that the part of the esophagus below the upper chest is

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<sup>36</sup> *Zeitschrift*, 49, 352.

<sup>37</sup> P. 351.

<sup>38</sup> P. 352.

<sup>39</sup> *Op. cit.*, 6.

the seat of the most indefinite sensations, those which are the hardest to localize. If the observer could in some way reach down his esophagus and touch the actual point stimulated, as he may do in localizations upon the skin, it seems probable that the accuracy of localization would be found to be much greater.<sup>40</sup>

### III. THE STOMACH

#### I. Thermal Stimulation

*Sensibility.* The stomach was stimulated with 25 cc. of water at various temperatures. Subsequently a special series with varying temperature was taken with the aim of determining whether the stomach is sensitive to cold or warmth, and, if so, at what temperatures. In this series 50 cc. of water were used. The combined results of all observations are given in Table IX. The grouping of observations made

TABLE IX

Sensibility of the stomach to temperature. Delayed sensations (probably due to the spread of the stimulus) are placed in parenthesis.

Stimulus Temp., °C.	Observers			
	B	D	F	G
0	cold	cool	cold	cold
10	cold	no temp.	no temp.	cold
19	cold			
20	cold	no temp.	no temp.	cold
23	cool			
27	cool			
30	cool	no temp.	no temp.	cold
31	no temp.			(cool)
25			no temp.	(cool)
39				(cool)
40	no temp.	no temp.	no temp.	
47	no temp.			(warm)
50	heat	no temp.	no temp.	warm
51				(warm)
55	heat			
60	warm	(warm)	no temp.	heat
70	heat	(warm)	warm	warm-heat-pain?
80	heat-pain			

<sup>40</sup> Meumann suggests that the esophagus might become educated in localization by the successive sensations occurring in the swallowing of food (*Archiv*, 14, 288f.). He does not seem to realize that the kind of localization under consideration requires that there should be an association between the internal sensations and processes representing the exterior. Becher has criticized his position (*Archiv*, 15, 358f.).



with different amounts of water and in different orders is justified by the fact that the stomachic conditions are never constant (from variation of stomachic contents), and also by the remarkable consistency of the data. Only those sensations that followed immediately upon stimulation are recorded, save in cases when no temperature sensations occurred until late. The quality of the delayed temperature sensations is entered in parenthesis.

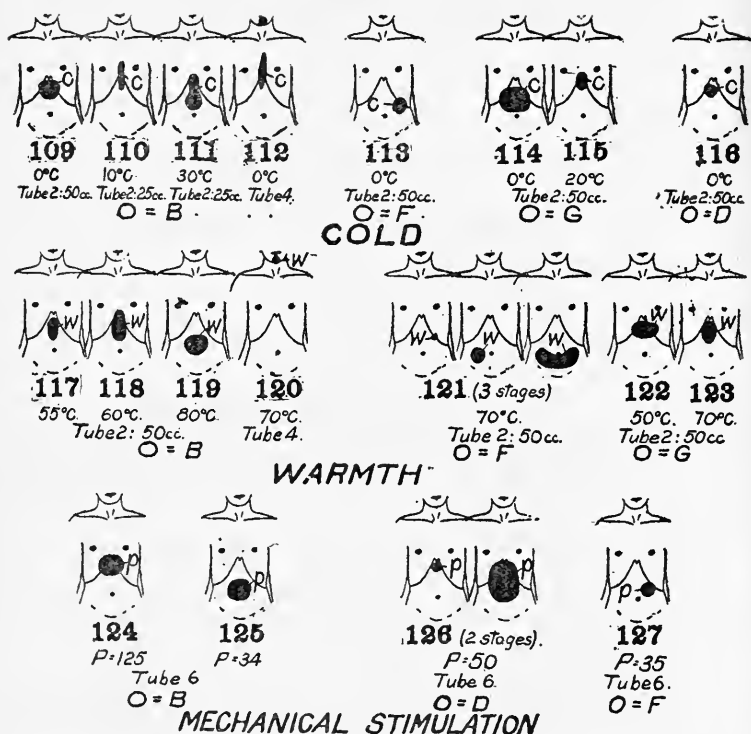
All the observers are sensitive to cold at  $0^{\circ}$  C. B feels cold at  $20^{\circ}$  and coolness at  $30^{\circ}$ . G feels cold at  $30^{\circ}$ . The delayed coolness that G finds for temperature as high as  $39^{\circ}$  may be due to conduction to the body-wall.

If we consider the delayed cool of G as not aroused in the stomach, then for all the observers there is a *Nullpunkts-temperatur*.

B once reports heat at  $50^{\circ}$ , although the result could not be duplicated. He always finds heat or warmth at  $55^{\circ}$  or  $60^{\circ}$ . G reports heat at  $60^{\circ}$  and warmth and heat at  $70^{\circ}$ . F finds warmth at  $70^{\circ}$ . D does not find either heat or warmth, except after some delay, for  $70^{\circ}$  or less. The experimenter hesitated to go above  $70^{\circ}$ , except in his own case, for fear of injuring the tissues. B finds thermal pain at  $80^{\circ}$ , and G suspects it but is not sure of it at  $70^{\circ}$ . We may say in general that B, F, and G appear to be sensible to warm stimuli, and that D shows no such sensibility within the limits of the experiment.

The sensibility of the stomach to cold and warmth was also demonstrated in B by the use of the water coil (tube 4) at  $0^{\circ}$  C and at  $60^{\circ}$  C. The electric heating coil (tube 3) did not give definite warmth at all.

*Localization.* The localization of cold from the stomach is shown in Figg. 109-116, and the localization of warmth or heat in Figg. 117-123. The points to which cold is referred seem approximately correct, though they are on the average too low. The localization to one side in Fig. 113 may be due to the actual position of the tube in the stomach, although F showed an individually marked tendency to refer incidental internal sensations to this side. Figg. 119 and 121 indicate the spread of the warmth. In Fig. 121 it is not unnatural that, in spreading, the warmth should no longer remain at the side, although it is surprising that it should lie entirely below the umbilicus. The only sensations referred to the throat were those of B (Figg. 112 and 120), in which the water coil (tube 4) was substituted for tube 2.



FIGG. 109-127.—LOCALIZATION OF THERMAL AND MECHANICAL STIMULI IN THE STOMACH

O = Observer; P = maximum intensity of pressure (cm. of water).  
c = cold; p = pressure; w = warmth.

*Quality of Sensation.* There can be no doubt that the cold experienced upon stimulation of the stomach is qualitatively like cutaneous cold. It is sometimes more peculiarly sharp and intense than the ordinary cold felt on the skin, and more like that from the glans penis. B describes the cold as "quite intense, sharp, very bright and pleasant, quite different, like paradoxical cold." D mentions only a "slight coolness." F reports "cold sensations, weak in intensity, overlying and in front of the bubbly sensations." G speaks of "sensations of cold, very clear and fairly intense," and later of a "more intense cold, low and narrow, combined with a weaker cold near waist line." She adds that she could not tell exactly where the boundaries of the areas were.

In order to get a comparison of the qualities of cold from the stomach and from the skin, the following experiment was arranged. The stomach tube was taken into the stomach, and a hot-water bottle wrapped in a cloth was placed on the abdomen and connected with another tube. Water was pumped into the bottle and into the stomach at the same time. At one time, cold water was pumped into the stomach, and water at body temperature into the hot-water bottle. At another time, the neutral water was given inside and the cold water outside. The observer described his experience. In the first trial, with the cold outside, B, the only observer, thought first that the cold came from the stomach; but later, when it had spread over a larger region than was usual for stomachic colds, he identified it as originating in the skin. When the temperatures were reversed, he was at first uncertain as to the origin of the cold, but later identified it as internal, because it was duller, heavier and less bright: adjectives which, he says, indicate a temporal, spatial, and intensive, but not a qualitative difference.

In a second trial cold water was injected simultaneously into the stomach and the hot-water bottle. B recognized the internal cold as coming first because it was accompanied by the typical reference of cold to the throat. He also believed that the external cold lasted longest because of the large area affected at the end of the trial and because of the pressure cues from the bottle. He was totally unable, however, to say at what point the internal cold passed over into the external or whether they both existed together at any time, and declared that the two experiences were identical in quality.

We may question the origin of the sensations of cold in internal stimulation in the stomach; but there can be no question that, wherever they originate, they are in quality like the sensations from the skin.

The sensations from hot stimuli are described both as heat and as warmth. B, for example, reports: "Heat suddenly begins and spreads in most alarming fashion; gets more intense and spreads rapidly (see Fig. 119). Almost felt as if whole front of my body were hot; like a flat sheet of heat a little below the surface. Very 'stingy,' bright and piercing, but not truly painful." Again he reports: "Besides the pressure, there is warmth from sternum down; then a general heaty warmth in the stomach region, almost burny, not so unlike pepper. After the trial, there is a general warm glow for some time." D, after an 8 sec. interval, reports a "warmth;" after 15 seconds, "different warmth;" after 32 seconds, "almost hot;" after 43 seconds, "still warm;" and after 78 seconds, "very indefinite, becoming a general glow." F, who had felt no warmth at all below 70°, found at this temperature a "little spot of warmth, quite warm, as small as the end of my finger," which "quite slowly spread over a large area. The more intense part of it forms a core about twice the size of a dollar." G states that there are "warm sensations to begin with, which become quite hot. They are located in the stomach; the area is longer than wide. They

become diffuse as time goes on, and are located more and more toward outside of body."

The experiment with the hot-water bottle described above was repeated with water at  $37^{\circ}$  and at  $60^{\circ}$ . The observer (B) was not deceived as to the location of the warmth, because on the outside the warmths were felt to agree spatially with the sensations of pressure from the bag, and on the inside, heat sensations were felt in the throat. B suspected, however, a qualitative difference between the two; the internal warmth seemed duller, more diffuse, and had something of the character of an ache in it. When warm water was injected both inside and out the internal sensations were interpreted as preceding the external, principally, as was the case with cold, because the sensations referred to the throat were felt first and the pressure of the hot-water bottle last. In the middle of the course the observer could not distinguish between the two complexes nor state where one began or the other left off. All the warmth was localized in the same region about the same distance beneath the surface of the body. The early warmths were more heat-like and more diffuse than the later ones. The presence of heat or even of an ache does not, however, argue for a qualitative difference, but merely for a greater effectiveness of the stimulus in the one case. Diffuseness may be spatial, dullness intensive. It is not conclusively shown that there is any qualitative difference at all.

*Seat of Sensation.* There can be little doubt that many of the delayed sensations following thermal stimulation of the stomach originate in the nervous organs situated elsewhere, perhaps in the skin or the body-wall. Often a warmth or a coolness will occur some time after the stimulation, and will spread well over the body, the whole temporal course occupying a minute or more. Whether the immediate sensations can be attributed to the stomach is not entirely obvious, especially as there is no qualitative difference between internal and external cold, and as the apparent difference in the case of warmth may be due to the fusion of an internal 'ache' with an external warmth. The extreme temperatures from  $50^{\circ}$  to over  $70^{\circ}$ , which are necessary to bring out the warm sensations immediately, may mean that no warmth can be felt unless the conditions are such as to promote rapid conduction. In favor of the position that temperature sensations depend upon conduction away from the stomach to more superficial tissues is the fact that their delay is decreased by increasing the temperature from  $50^{\circ}$  to  $80^{\circ}$ . Finally the qualitative similarity—not to say identity—of the experiences dependent upon external and internal thermal stimulation is evidence for their common seat. The case is perhaps still unproven, but the writer inclines to the belief that the actual tissues of the stomach do not mediate the sensations of cold or warmth,

but that these sensations arise upon internal thermal stimulation by conduction to the body-wall.<sup>41</sup>

## 2. Mechanical Stimulation

*Sensibility.* In all observations both pressure and pain sensations were elicited by inflation of the bladder of tube 6. The intensities, expressed in cm. of water required to produce a just noticeable pressure and a just noticeable pain, are as follows:

	B	D	F	G
Pressure. . . . .	12	10	20	25
Pain. . . . .	35	50	35	35

It is again difficult to state whether these sensations originate in the stomach or whether they come from surrounding tissues as the result of distension of the stomach. In extreme inflation, the pressure spreads down into the abdomen, and when the air is released there is a general feeling of relaxation. These sensations undoubtedly come from the body-wall, and possibly from other organs, and there is no assurance that the lighter pressures are not also mediated by the surrounding tissues.<sup>42</sup>

*Localization.* The localization of pressure in the stomach is shown in Figg. 124-127. The reference is usually to a point below the stomach, sometimes to one as low as the umbilicus. This fact is in accordance with the belief of most people that the stomach is well down below the ribs, as well as with the recent anatomical conclusions.

<sup>41</sup> This conclusion is in accord with those of Becher (*Zeitschrift*, 49, 350), Hertz, Cook, and Schlesinger (*op. cit.*, 483), and of Hertz (*op. cit.*, 7), although the last writer admits that the stomach may "rarely" be sensitive to heat and cold (p. 9). The present writer plans a further detailed study of this point for the future.

<sup>42</sup> Hertz states that the stomach is insensitive to tactile stimulation (*op. cit.*, 3), but that the sensation of 'fulness' is experienced at a pressure of about 18 cm. of water (p. 19), a value that accords closely with our own. Becher, however, insists that the stomach is insensitive to pressure and that the sensation of fulness is due to expansion of the body-wall (*Zeitschrift*, 49, 356f.). Meumann speaks of "eine charakteristische Empfindung von Fülle und Druck im Magen" (*Archiv*, 9, 52f.), but probably does not mean typical pressure. He is interested chiefly in the *Verdauungsgefühle* and takes pains to distinguish the sensations of *Fülle* and *Leere* from the *Druck- und Zugenmpfindungen* of the skin of the abdomen and also to separate the feelings of *Fülle* and of *Sättigung* from each other (*Archiv*, 14, 292, 295). He ascribes hunger partially to the stomach (*Archiv*, 9, 152).

*Quality of Sensation.* The following reports were made upon the quality of the stomachic sensations.

B, Max. press. = 25 cm. "Dull ache, fairly well down; distinct from the mere pressure sensations above. The ache is sharply painful, but is something like a sensation from an object stuck in the throat. It seemed perfectly simple and elementary, and like pressure plus pain. It is something like hunger, but more diffuse and cloudy. The boundaries are indefinite."

B, Max. press. = 34 cm. "At first I begin to feel pressure. General distension got gradually more intense and very definite; similar to sensation of fullness after a big meal and somewhat like cutaneous pressure, *i. e.*, not at all 'achy.' Quite definite. Perhaps it is cutaneous pressure; my body-wall feels bulged out. Later, I feel as if I were going to burst. I think the sensations on release of pressure are entirely cutaneous."

D describes the stomach pressure as "large" and "bloaty."

F, Max. press. = 30 cm. "Very weak muscular-pressure-like sensation. Then I got a pole of pressure just a little to the left and above umbilicus, moderate intensity. Pole increases in intensity and gets larger. Finally gets painful. Pressure very intense. Pain more scary than intense. Made me break into sweat. Strain sensations came in somewhere quite superficially, way up near the sternum; a bit colicky and more superficial than the strain sensations accompanying the pain above. Could feel the outside body-surface expand; probably tactual sensations."

G gets slightly painful sensations of "crampy nature, neither stabbing nor sore," finally however becoming "definitely crampy and muscular."

It is evident we have found no new quality of sensation by mechanical stimulation of the stomach. The pressures and dull pains are of the same order as those felt in the esophagus and, to a certain extent, below the surface in any portion of the body.

### 3. *Electrical Stimulation*

*Sensibility.* The stomach proved to be variously sensitive to electric shock with tube 7. The minimal stimulation occurred within the following intensities (Kronecker units); the variation was due to chance differences of contact in the stomach or of the short-circuiting effect of the acid in the stomach.<sup>43</sup>

B	D	F	G
3750-7600	6250-10250	3750-4950	4950-10250

*Localization.* The localization of these sensations is best shown in the results of a special series upon B. Tube 7 was used and the coil was set at 4950 units. Stimulations were given with the tube at 45, 50, 55, and 60 cm. from the teeth,

<sup>43</sup> Becher reports that the stomach is insensitive to electrical stimulation (*Zeitschrift*, 49, 356).

five trials in each position, twenty in all. The tube was pulled up and forced down each time and the positions were selected in a random order. The results, which show a degree of uniformity greater than that for the esophagus, follow.<sup>44</sup>

Position of tube.....	45	50	55	60
Average localization .....	38.2	42.8L3.6	50.2L3.6	51.8Ro.6
Mean variation .....	1.0	1.3L1.1	1.0Lo.5	1.8R2.5

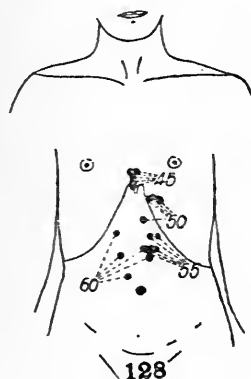


FIG. 128.—LOCALIZATION OF ELECTRICAL STIMULI IN THE STOMACH

Points of reference for 5 stimuli for each of 4 lengths of tube within the stomach, viz., 45, 50, 55, and 60 cm. from the teeth.

Fig. 128 shows the localizations diagrammatically. The exact error of localization can not be determined because the exact position of stimulus is indeterminable. Neither can the position of the stomach be stated with certainty,<sup>45</sup> nor can the course of the tube within it be known. The localizations, however, for a given length of tube group themselves quite closely, those for the longest lengths lie farthest from the cardiac region, and the course of variation—first downward and then to the right—is that which might be expected in the most usual stomach.

<sup>44</sup> Meumann has already maintained that with palpation one may come to localize the stomachic sensations very definitely (*Archiv*, 9, 52).

<sup>45</sup> "There is no organ in the body the position and connections of which present such frequent alterations as the stomach" (Gray, H., *Anatomy, Descriptive and Applied* (Spitzka), 1913, 1005). Recent radiographic work has shown that the stomach may frequently extend as low as the umbilicus or even entirely below it, and that both position and size vary very greatly with the individual and with internal conditions in a single individual. Cf., e. g., the radiographs reproduced by v. Elischer, J., *Ueber eine Methode zur Röntgenuntersuchung des Magens*, *Fortschr. a. d. Geb. d. Röntgenstrahlen*, 18, 1912, 333ff. No wonder that Herr B. in Becher's experiment appeared to localize "sogar fast unterhalb des Magens."

*Quality of Sensation.* The observers had great difficulty in describing the quality of intense induction shocks. The weak ones were, however, described as faint catches or jerks, and the observers all inclined to the belief that there was no difference from the sensations that would be felt under similar conditions on the periphery. The shocks were much brighter and less ache-like than those in the esophagus.

It may seem that the production of kinesthesia-like sensations in the stomach by weak electrical shocks argues for the sensibility of the stomach itself. The proof, however, is not positive; for the more intense shocks sometimes made the body wall twitch; and no shock greater than 3750 units can well be considered as so weak that it would be unlikely to spread (see p. 32).

#### 4. Chemical Stimulation

*Alcohol.* Various concentrations of alcohol were blown into the stomach with tube 2, five cc. at a time. Faint warmth was felt for 60% and also for 100%. With the latter, coolness was felt below the sternum at first, and then a very faint warmth, which is a little like heat, that is to say, a faint burning which shifts intermittently over a fairly large area. Later, there is "an uncomfortable feeling," which consists of a dull ache and a pressure, such as one has when sick at the stomach, only much fainter.

*Hydrochloric Acid.* Both 5% and 10% HCl produce, in the stomach, sensations very much like those constituting the ache of hunger. B reports for 5% "hunger, or a strong intense or diffuse ache, getting fainter."

Twenty *per cent.* HCl produces dizziness in the head, in the region in which the "ice-cream pains" are localized, warmth in the face, a feeling of tenseness in the region of the stomach, a vague ache in the same region, and tingling, 'achy,' 'nervous' sensations in the arms.<sup>46</sup> Later the ache in the stomach changes, and is recognized as nausea, although its similarity to hunger is also noted. In fact, the observer thinks that the difference between hunger and nausea is not qualitative.<sup>47</sup>

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<sup>46</sup> Hertz, Cook, and Schlesinger do not find the stomach sensitive to HCl (*op. cit.*, 486); but Becher mentions several experimenters who obtained sensory responses with fairly strong solutions (*Archiv*, 15, 365f.).

<sup>47</sup> The writer has been at some pains to gather introspections upon hunger, nausea, thirst, and other organic experiences. He reserves their publication until such time as he shall be able to prepare a more thorough introspective study of organic sensations. The adequacy of HCl as a stimulus to hunger suggests that it must set up the hunger contractions (*cf.* Cannon and Washburn, *op. cit.*, and Carlson, *opp. cit.*). The present writer found that HCl in the intestine produced contractions and the call to defecation. Cannon, however, says that it produces normal peristalsis in the stomach and stops hunger.



*Mustard.* B swallowed six capsules, each containing one-half gram of mustard, at intervals of two minutes. At first, there were aches in the upper part of the esophagus, very dull and pressure-like. Later came pains and swimming sensations in the head, referred principally to the eyes and to the back of the head. Next the ache in the esophagus became intensely warm and assumed a very uncomfortable, insistent, burning character. It was shot through by a core of sharp stinging burns localized in the throat. Next came muscular sensations in the throat, "as if the esophagus were stopped up." Later the dull 'achy' pains spread to the arms, the heart, and other deep parts of the body.

*Pepper.* B swallowed successively at two minute intervals six capsules, each containing one-third of a gram of black powdered pepper. This experiment resulted in warm sensations in the throat above the clavicle. The sensations were faint, but very 'stingy.' Later the warm got more intense, and was accompanied by an 'achy' pain, also referred to the throat. Much later, it became hot. The heat and pain extended all the way from the throat down to the region of the stomach, remaining however much more intense above. The heat complex, which was very much like a cutaneous heat, including the usual 'sting' and occasional brighter 'shots' of pain, lasted a longer time. Even half an hour later, when it had become intermittent, heat returned frequently upon swallowing. Still later, over an hour after swallowing the pepper, warmth continued to be felt intermittently in the throat.<sup>48</sup>

*Peppermint.* One-half cc. of 10% oil of peppermint in olive oil was swallowed in a capsule by B. At first cool sensations in the throat and the upper esophagus were felt. Then there was a very vague warmth accompanied by the typical esophageal ache. Still later the whole throat began to burn with a complex of warmth, 'sting,' ache, and pressure, very well fused, and with cold, not so well fused with the others. The burning feeling is the characteristic feeling of rawness, a sort of 'cold heat.' With it there is a 'drawing' sensation in the region in which 'ice-cream pains' are felt in the head.

#### IV. THE RECTUM

The experimental work upon the rectum and intestine was performed upon B only.

##### 1. *Thermal Stimulation*

The sensitivity of the rectum to temperature was studied by means of tube 9 and the apparatus of Fig. 1. Tube 4 was also used in the rectum. The points selected for stimulation were those 5, 10, and 15 cm. respectively from the anus. In being pushed in even as far as 15 cm. the tubes sometimes pressed against the intestinal wall, causing considerable discomfort. At first the experimenter attempted to work at

<sup>48</sup> Meumann's description (*Archiv*, 14, 293f.) does not indicate that the sensations were referred to any region other than that of the stomach.

20 cm., but the difficulty of forcing in a heavy and not too flexible tube led him to give up the attempt. At one time 30 cm. of tube 9 were inserted, with the result that the end of the tube pressed against and distended the body-wall at 62 L 10 (Plate II). The distension was somewhat painful and alarmed the observer, so that, although observations were taken at this point, no attempt was made to repeat them.

The following extracts show the results of the use of tube 9.

0° C. 15 cm. "A slight coolness which spread a little to both sides was felt at the anus. It was very faint and soon disappeared."

0° C. 10 cm. "A thrill of cold that I can hardly localize. I visualized it as several silver threads running across below the surface of the nates. Cold was very intense, but small in area; always keen; lasted a very short time."

0° C. 10 cm. "Several tiny thrills of cold, very difficult to localize; I referred them to the glans and other parts of the penis. Later, after an effort to determine their position, they seemed to lie out in the air away from that part of the body. It seems as if the reason that I can not place them on the body is that, although they seem to be in certain places, the attention to these places (*i. e.*, to the other sensations referred to these places) seems to give a tactual group without the cold sensations, which are somehow different from the tactual."

0° C. 5 cm. B finds "several quite intense, little thrills of cold," which he localizes in the left inguinal region and in the left thigh. He observes again that the tactual sensations which accompany attention to these parts, as well as those that occur when he touches these places with his hand, seem to be peculiarly separate from the cold sensations and unconnected with them. He is inclined to believe that this lack of connection is nothing else than the failure of the two groups to become clear at the same time.

With the water coil of tube 4, B finds, for all positions, a coldness in the region of the anus, which does not occur until the walls of the tube begin to become slightly cool. At 15 cm. he reported a very faint sensation, which he thought at first was going to turn either into cold or into heat. He was not, at any time, able to identify it with confidence, but thought that it might be a weak form of the 'sting' of heat. The same experience occurred at 10 cm. At 5 cm. a slight coolness above the anus was felt.

It appears probable, then, that the rectum is sensitive to cold only in the region of the anus. Cold water, injected into it, gives very intense, bright, cold sensations, which are referred to various places in the region of the perineum. Cold stimulation of the upper rectum produces an indefinite sensation that belongs probably to the pain family, possibly a form of 'thermal pain.'

For warm stimulation 50 cc. of water at 50° C. and at 70° C. were injected through tube 9. It was found that warmth, unlike cold, tended to produce the call to defecation. The call was very marked at 50° C., and at 70° C. it was so violent,—involving intense, griping pains, a general bodily disturbance, and a very strong impulse toward

a motor response,—that it interfered with the introspection. The amount of water in this series was accordingly reduced to 25 cc. The call still remained quite definite, but the pains decreased very much.

At 5 cm. and 50° C., B felt a warmth "quite intense although not heat." It was localized deep in the body, well toward the back and about two centimeters above the coccyx. At the same time, with 25 cc. at 70° B reported a heat, which fused with the muscular sensations involved in the call of defecation, and which turned later into warmth.

At 10 cm. and 50°, B was "surprised at the absence of temperature." At 70°, however, he reported that, after a period of faint pressure, there developed "a sudden burning heat, with pain, followed at once by the call to defecation." Twelve seconds later the heat became warmth, and then gradually faded out during the minute following.

At 15 cm. and 50°, B found no positive temperature, although he remarked that it was very difficult to distinguish between very faint warmth and light pressure. At 70°, warmth was felt in the region of the anus.

In the exceptional case, mentioned above, in which the tube was pushed in to 30 cm. and forced against the body-wall at that point, no temperature sensation was felt for water at 50° C. at the place where the tube ended, although there was general warmth at the anus. Sensations of pressure were definitely referred to the place at which the tube ended. At 70°, besides the warmth at the anus and a general warmth, around the umbilicus, heat from the end of the tube occurred in the same place as the pressures.

With the water coil (tube 4), 25 cc. of water at 50° and at 60° C. were used. A single trial at 70° proved very painful.

At 5 cm. and 50° there was only a very faint sensation which B thought might be either pressure or warmth. At 60° there was pain immediately, and later warmth and heat at the anus.

At 10 cm. and 50°, there was a faint warmth, somewhat delayed, at the anus. For 60° B again hesitated between warmth and pressure, but, as the sensation became more intense, decided in favor of warmth, which appeared to be localized in the scrotum and perineum until he touched those parts. Then the warm sensation seemed to recede farther in. At 15 cm. for both 50° and 60°, warmth was felt after a time at the anus. In the second case, there was also pain and the call to defecation.

We may conclude, then, since warmth is not felt until there has been sufficient time for the tube to become warm, and since in the trials with tube 9 the rectum does not respond with a sensation of warmth except very near the anus, that the lower part of the rectum only is sensitive to temperature. The sensitivity of the anus itself to both cold and warmth was evinced again and again by the fact that cold or warm water, which had been injected and which no longer produced any sensation, felt very cold or warm as it passed the anus in being expelled. The heat felt at 70° and 30 cm. from the anus does not necessarily indicate that the intestinal tract is sensitive to warmth at that place, for 50° did not produce

any temperature there, and the end of the tube was so close to the body-wall that conduction to superficial organs might have been almost immediate with a higher temperature.

The immediate response of the rectum with pain to a temperature of 60°, and the fact that this pain was of the quality of most internal pains, indicate that what rectal sensitivity to thermal stimulation exists is of a painful character.<sup>49</sup>

Warm water in the water-coil did not produce the call to defecation as strongly as did the warm water in direct contact with the rectum. Warmth probably makes the distension of the bowel by the water more effective as a stimulus for defecation, but is inadequate alone.

## 2. Mechanical Stimulation

Mechanical stimulation of the rectum was accomplished by the use of the bladder of tube II. The apparatus of Fig. 2 was used. For all positions of the tube, the resulting experience was much the same. At first a very faint pressure was felt. The liminal values were approximately 30 cm. of water for the position of 5 cm. from the anus; 24 cm. of water for 10 cm. from the anus; and 20 cm. of water for 15 cm. from the anus. This pressure is described as dull and diffuse and is localized in the region of the rectum; that is to say, B has a visual image of the place, deep in from the back and above the coccyx. Later the call to defecation is produced (at 50 cm. of water for 5 cm. from the anus; at 30 cm. for 10 cm.; and at 32 cm. for 15 cm.). The call is very complex, and gets more so as the pressure is increased. It is produced less easily near the anus. It always involves pressure, colored with a dull ache, similar to the ache-like pressures of the esophagus, and localized in the rectum. These pressures fall into a pattern, which means, for B, movement of the bowels. There are also "general pressures" in the abdomen, which come when the call gets at all intense. They are localized in the region of the umbilicus, but extend well over that portion of the body. They are less ache-like than

<sup>49</sup> These conclusions accord with the original results of Weber (Weber, E. H., *Der Tastsinn und das Gemeingefühl*, Wagner's Handwörterbuch der Physiologie, iii, 562ff.) and with those of Hertz, Cook, and Schlesinger (*op. cit.*, 485). Head, Rivers, and Sherren, on the contrary, hold that the intestine is sensitive to extremes of temperature (*op. cit.*, 112), and Becher observes that intestinal cold is felt, but localized in the anterior abdominal wall (*Archiv*, 15, 375f.). Meumann insists on the general sensitivity of the intestines, but does not directly consider thermal sensibility (*Archiv*, 14, 310).

the other pressures. Besides the pressures, there are different pains which range from a dull, diffuse, and never more than moderately intense ache to a sharp keen intense pain, which may be either massive or bright (probably an extensive difference). B found it convenient to think of the organic pains as arranged in a two-dimensional manifold, in which an intensive change from 'weak' to 'strong' was paralleled by a qualitative change from 'dull' to 'bright.' Extensive differences, for example those involved in the distinction between fine and heavy, diffuse and clear-cut, occurred independently of the intensive-qualitative relation.<sup>50</sup>

In the call to defecation, all sorts of pains were involved,—bright, intense, fine pains referred to the penis, scrotum, perineum, and abdominal wall, as well as deep, heavier aches within the abdomen. The scrotal pains were usually slightly more massive than the tiny, wiry, shooting pains in the penis and perineum.

We cannot say whether these pressures originate in the rectum itself. Not only is the rectum distended by the rubber bladder, so that adjacent tissues are affected, but the stimulations seem to produce movement over a large portion of the colon, so that the secondary sensational response is widespread.

The pains from the rectum may come from the canal itself or from the peritoneum. The definite ache-like pains, that occurred when the tube was forced against the wall of the rectum, and that could be brought out with very great intensity by pressing the body-wall against the end of the tube at 30 cm., may have come from the intestine, but were more probably caused by the distension of the peritoneum.<sup>51</sup>

### 3. *Electrical Stimulation*

For electrical stimulation tube 12 was used with the apparatus of Fig. 3.

The rectum is more sensitive near the anus than farther in. The setting of the coil required to produce a just noticeable sensation was 1500 units for 5 cm. beyond the anus and 3750 for 15 cm. Shocks just slightly greater than these liminal values were used as stimuli.

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<sup>50</sup> The qualitative classification of organic and cutaneous pains needs to be worked out. A great variety of stimuli adequate to different sorts of pain are indicated in this paper. The writer plans at present to utilize them for a qualitative study.

<sup>51</sup> Our results accord with those of Becher (*Archiv*, 15, 373f.) and Hertz (*op. cit.*, 28ff.).

In quality, the electric shock produced a sensation like muscular pressure. B identified it absolutely with muscular pressure for points well within the rectum. Just beyond the anus, however, it seemed to him to contain more of the brighter quality of cutaneous or subcutaneous pressure. Here there were also traces of an ache, and the whole experience was more or less like an electric shock on the skin. Pain occurred from stimulation well within the rectum only once, and was then described as the "bright, diffuse pain usual in electric shocks."<sup>52</sup>

The sensations resulting from shock were localized fairly correctly for the position just beyond the anus. When the tube was 10 cm. in, they were again felt in about the same position, *i. e.*, deep, and slightly above the coccyx. At 15 cm., however they were referred out into the flesh of the nates. The sensations seemed to extend in a long transverse line at one time across both nates, at another time upon the left natis only, and a third time in a smaller region to the left of the median line.

#### 4. Chemical Stimulation

*Alcohol.* Ten cc. of 30% alcohol, injected with tube 10 at 15 cm. from the anus, resulted at once in stinging pains and the call to defecation, and later in a widespread bodily reverberation, including shivers in the legs and sweat in the chest. Expulsion of the alcohol was exceedingly painful, for it made the anus smart and burn.

*Hydrochloric Acid.* Ten cc. of 5% HCl introduced at 15 cm. from the anus gave "warm, stinging sensations, in which the sting is bright, somewhat like cutaneous prick." The sensations were localized 5 or 10 cm. within the rectum. Two *per cent.* gave no relevant sensations, although the call to defecation followed shortly.

At 10 cm. from the anus, 5% HCl produced a complex muscular pattern in addition to the warm, stinging sensation described above. Four *per cent.* gave a cool thrill at first and later the stinging sensation. In both cases the call to defecation was very strong indeed, involving pressures all over the abdomen.

A faint sting was felt preceding the call to defecation with 10 cc. of 2% HCl were injected 5 cm. beyond the anus. Expulsion of the acid was very uncomfortable since it produced "raw" sensations in the anus. These sensations were, however, not nearly so intense as those occurring in the expulsion of the alcohol.

*Mustard.* A mixture of water and mustard (5:1) was injected at 15, 10, and 5 cm. beyond the anus. There were no results beyond a weak call to defecation.

*Pepper.* Pepper suspended in water (1:100) was introduced at the three usual places. Aside from the sensations involved in the call to

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<sup>52</sup> Becher's description of electrical stimulation within the rectum is similar to the present one. He finds dull shock sensations at 20 cm. and bright sensations at the anus (*Archiv*, 15, 374f.).

defecation, which was not intense and occurred in all cases, little thrills of intense cold were experienced. These were referred to the neighborhood of the sphincter and to the anterior surface of the body just above and to the left of the *symphysis pubis*. They occurred immediately and were probably caused by the temperature of the water, which was at 18° C.

*Peppermint.* Five cc. of oil of peppermint were injected at 15 and 10 cm. from the anus. In the first case, a faint coolness was felt immediately in the rectum. Later, on expulsion of the oil, there were cold burning pains at the anus. The anus felt raw and irritated for some hours afterwards. Apparently the upper rectum is not sensitive to the oil.

## V. THE COLON

### I. *Thermal Stimulation*

An enema of about a quart of water at 50° C. was given in order to see if there were any temperature sensations above the rectum. The usual pressures and pains involved in a violent call to defecation occurred, but there were no temperature sensations referred to any place above the rectum. A second enema at 60° C. produced so much pain that it was necessary to discontinue it before any general warmth had been noted.

An enema of a pint of water at 0° C. resulted successively in cold in the anus, an ache in the penis, an ache-like pressure in the rectum, and finally an indefinite sensation in the region of the umbilicus. This 'indefinite sensation' B describes as "a faint sting, which was possibly cool. It is very much like the sting in 'cold heat,' *i. e.*, the sensations of heat that one gets from a cold stimulus at times during nerve regeneration. It is, however, very much weaker than the cold heat to which I am accustomed."

### 2. *Pressure*

The various pains and pressures involved in the call to defecation have already been noted in the description of sensations from the rectum (p. 53). The inflation of the whole bowel with air failed to produce anything new. B reported definitely that the pressure was always of a dull muscular variety, always diffuse and never bright. The pains were the usual aches of distension, which get brighter and sharper with increase of intensity (p. 53). A moderate amount of inflation resembled the normal feeling of fullness which ordinarily represents the need for defecation. Greater intensities resulted in typical "belly pains."

## SUMMARY

The esophagus is sensitive to warm and to cold stimulation throughout its length. Mild stimuli give rise to sensations which are qualitatively like the sensations of cutaneous warmth and cold. Stimuli of approximately  $60^{\circ}$  C. give rise to heat, which probably includes a paradoxical cold. Extreme cold or heat results in pain. The temperature sensations are sometimes referred to the throat from lower regions, and the thermal pains are often localized in the head.

Mechanical stimulation, which results in the distension of the esophagus, is sensed at weak intensities as pressure and at greater intensities as pain. Pressure is more readily elicited by rapid than by slow distension. In quality the pressure is like the muscular pressure, and the pain like the deep pain, of the forearm.

The esophagus is everywhere sensitive to bipolar faradic stimulation, and increases in sensitivity toward the pharyngeal end. The experience is qualitatively similar to the pressure-pain complexes of electric shocks upon the periphery.

The esophagus is sensitive to alcohol and hydrochloric acid, but not to oil of peppermint or mustard or pepper in suspension.

The sensations from the esophagus are in general referred either to the region below the sternum or to the throat. The amount and direction of reference are fairly constant for any given position, and the amount is greatest mid-way between the clavicle and the nipples. At this point sensations are referred in either direction, or sometimes in both. Transverse reference is never more than one cm. from the median line. This tendency to localize esophageal sensations upon the soft portions of the body suggests that these portions come by virtue of their mobility to be connected in experience with internal parts.

Cold and warm stimuli in the stomach produce the corresponding sensations, which are identical in quality with cutaneous cold and warmth. It is probable, however, that these sensations do not arise from the stomach itself but from the superficial tissues. Extreme warmth gives rise to pain which may originate in the stomach.

Distension of the stomach gives rise to sensations of dull pressure and, when extreme, of pain.

Electrical stimulation of the stomach gives rise to sensations characteristic of electric shock; the minimal intensities are so great, however, that there is no assurance that the current does not spread to surrounding tissues.



The stomach is sensitive to strong concentrations of alcohol, to oil of peppermint, and to pepper and to mustard in suspension. Hydrochloric acid may produce hunger pains, and in strong concentrations dizziness in the head.

Localization of strong electric shocks within the stomach is quite accurate, much more so than the localization of shocks within the esophagus. The points of reference follow the probable position of the stomach in both the longitudinal and transverse dimensions.

The anus is sensitive to both warmth and cold. The rectum is probably insensitive to both as such; water of 60° C., however, produces pain.

Distension of the rectum produces the call to defecation in all degrees of intensity. The disturbance in this complex experience is so great as to mask any specifically local sensations of pressure that may be set up. In intense degrees the call involves pressures and pains in the abdomen and the genital organs.

The rectum is sensitive to electrical stimulation. It is most sensitive at the anal end.

Alcohol and hydrochloric acid are sensed in the rectum. Oil of peppermint, pepper, and mustard are not. The anus is sensitive to all but the mustard. Hydrochloric acid also produces the call to defecation.

Hot and cold enemata fail to produce thermal sensations in the colon. They do produce pain. Pressures and pains result from distension, but can not be attributed to the intestine itself.

## A NOTE ON THE RETENTION OF ACQUIRED CAPACITIES<sup>1</sup>

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It is a familiar fact that aptitudes once acquired, if not indeed inherited tendencies, are weakened by lack of use. *Ex desuetudine amittuntur privilegia*, remarked Friar John to Panurge. The process may be observed at all psychological levels. Some of us may still call to mind the feeling of inadequacy with which, after a student's protracted summer of intellectual inactivity, we again faced the tasks of the laboratory and the lecture-room. It requires a few days to get one's knowledge under discipline again. In the more highly intellectual games, where the objective test of capacity is quite immediate, the necessity for constant practise is fully recognized. When, after a period of neglect, an ordinary player again resumes such pastimes as chess or whist, he is not surprised if he occasionally misses a simple combination or overlooks a not too obvious finesse; errors that are understood, and which at a former period could not have occurred. Such events as these demonstrate how relative a thing is the term "to forget." We may speak of a response as "forgotten" when it is not called up in the presence of some associative milieu that would have called it up at some previous time. But another associative milieu may still call it up, and only when this is no longer possible, is a response absolutely forgotten. Theoretically one might suppose that forgetting begins when immediate learning ceases, but there are experimental observations to indicate that this is not quite the case, there being a short period, 15 seconds or so after the stimuli, during which the effectiveness of the memory increases. The fewer and simpler associations required, the less will naturally be lost. The writer probably plays casino at least as well as when last remembering to have played it, fifteen years ago; auction bridge, not as well as two months ago.

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<sup>1</sup> Read at a meeting of the Section of Anthropology and Psychology of the New York Academy of Sciences, February, 1913.

In the motor sphere, losses through disuse may be the product either of less adequate co-ordination, or of deterioration in the muscle. Both seem to have a part. When on the resumption of lawn tennis in the spring, one plays well for a set or so and then goes to pieces through fatigue sensations, the former factor appears to preponderate; and when it takes some moments to again get into the swing of some move with the punching bag, the latter. On the other hand, muscular fatigue is often a secondary result of inadequate co-ordinations; witness how a slight but practised woods-walker can always outlast a sturdier but less experienced companion. Comparison between the retention of intellectual and motor memories is possible with quantitative measurement; it seems also that after voluntary learning has ceased, there is more automatic *Einprägung* (of the "learning to skate in summer" type) with motor associations than with intellectual ones.

Proper mental or motor response to a situation, once learned, may through disuse, appear as practically entire loss, as partial loss, or as mere delay. In mental processes it is not easy to distinguish between the latter two, because the delay brings about changes in the general associative set in which the particular association required is more likely to arise. Partial loss is more especially a high level process, delay one of low levels. An especially pure form of this delay is seen in the decreased speed of practised simple reaction. A thousand observations with the writer during December, 1906, gave an approximate average of 105 with an m. v. of 10; 48 observations recently made at Harvard, though under different conditions of stimulation, an average of 123 and m. v. of 9.2.<sup>1</sup>

For experimental purposes it seems more desirable to measure these decreases of functional efficiency in terms of delayed responses than in terms of lost responses. That is, to consider responses which have no likelihood of being lost under the experimental conditions, and measure the amount by which the responses have been delayed, from any cause incident to the disuse of a former practise ability. Two series of practise experiments, already described, afforded the conditions for the present observations. The first of these concerned the tapping test, the second the functions of addition and cancellation of digits.

Two subjects had taken part in the practise experiment

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<sup>1</sup> The first of these observations was made with Professor Henmon; for the second the writer is indebted to Dr. Langfeld.

with the tapping test, but unfortunately only one remained accessible. The following table presents figures by which the gross rates for thirty seconds in the recent series may be compared with those of  $5\frac{1}{2}$  years previous.<sup>2</sup>

TABLE I

NUMBER OF LAPS IN 30 SECONDS FOR 5 SUCCESSIVE SERIES DAILY AT  
2' 30" INTERVALS

Series	RIGHT HAND						LEFT HAND					
	Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912		Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912	
	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
1*	194.6	3.6	209.5	4.1	205.7	5.5	184.6	3.7	197.3	5.1	186.3	5.5
2*	195.4	4.4	213.7	3.7	208.3	5.8	183.8	4.7	202.3	3.8	189.6	4.0
3*	197.7	3.9	221.5	3.7	216.5	5.5	188.4	6.2	207.7	3.1	195.1	6.3
4*	200.8	3.1	223.6	2.2	218.8	5.8	188.9	4.5	211.9	5.5	198.3	5.6
5*	201.1	5.5	224.9	3.7	221.7	4.7	189.1	4.7	212.4	3.1	201.5	6.9
Av.	197.9	4.1	218.6	3.5	214.2	4.9	187.0	4.8	206.4	4.1	195.2	5.2

Comparing averages for ten consecutive days, there has been but a slight decrease in the efficiency acquired in the earlier experiments. The ten recent ones average not far from the last ten days' practise of 1907. A striking subsidiary fact is that the warming up phenomenon between the successive 30" series, practically absent until brought out by practise, has been in amount practically unaffected by the five years' intermission. Here the 1912 experiments are practically identical with those of 1907, even to the series showing the greatest warming up gain, the third in each hand. But while during the 1907 practise the left hand gained practically the same as the right, it has during the five intervening years lost very much more, both absolutely and relatively. Probably in a right handed person, the ordinary activities, in which it has the greater share, preserve its specially acquired capacity better than that of the less preferred hand. Finally, the variation of corresponding series on different days is distinctly greater than at any time during 1907. The effect of the 1907 practise was to reduce this variation, so that this is a practise effect that has been more than lost.

To consider the results from the standpoint of fatigue, the subjoined table gives the figures for the six successive 5" intervals of each 30" series, tabulated for the same set of data as before:

<sup>2</sup> The earlier results have been reported in this JOURNAL, XIX, 1908, 437-83.

TABLE II

NUMBER OF LAPS IN EACH OF THE SUCCESSIVE 5" INTERVALS. AVERAGE OF 50 CASES OF EACH INTERVAL, 5 FOR EACH OF TEN DAYS. THE M. V. GIVEN THE DAY TO DAY VARIATION OF THE AVERAGE OF THE 5 CASES FOR THAT DAY.

Series	RIGHT HAND						LEFT HAND					
	Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912		Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912	
	Av. M.V.		Av. M.V.		Av. M.V.		Av. M.V.		Av. M.V.		Av. M.V.	
0' - 5'	36.4	1.04	40.8	1.06	39.8	.80	34.2	1.06	38.1	.76	36.3	1.10
5' - 10'	34.7	.52	38.8	.50	37.7	1.00	32.7	.79	36.1	.69	34.3	1.14
10' - 15'	33.3	.60	36.5	.62	35.9	.94	31.2	.72	34.6	.54	32.4	.96
15' - 20'	32.1	.59	35.1	.48	34.4	.70	30.4	.64	33.3	.50	31.2	.82
20' - 25'	31.1	.54	33.9	.51	33.5	.96	29.5	.73	32.4	.56	30.2	.72
25' - 30'	30.6	.32	33.4	.52	32.8	.84	28.9	.46	32.0	.58	29.7	.64

The situation is different in the two hands. The right hand, which as we just saw, has lost less, loses more in the earlier intervals than it does in the later; the left hand has its greater loss in the later ones. In consequence, while the right hand is in the 1912 experiments slightly less fatigable than at the close of the 1907, the left hand is rather more so; that is, it has lost more than the right both in respect to initial maximum and to endurance. The  $f$ 's express this endurance for the different groups of experiments as follows:

TABLE III

AVERAGE INDEX OF FATIGUE (b) FOR THE SUCCESSIVE 10 DAY PERIODS

	Days 1-10, 1907	Days 11-20, 1907	Days 21-30, 1907	Days 1-10, 1912
Rt. Hd.	89.0	88.2	87.0	87.8
Lft. Hd.	89.5	89.2	88.4	87.1

As is seen, the 1907 practise tended to increase the initial rate of this subject at the expense of his endurance; the endurance in the left hand has continued to weaken, while in the right it has been somewhat restored.

Among the most positive results of the 1907 experiments was that the later series of any given day became, as a result of practise, more immune to fatigue than the earlier. This feature is scarcely present in those of 1912, as is seen in the following table:

TABLE IV

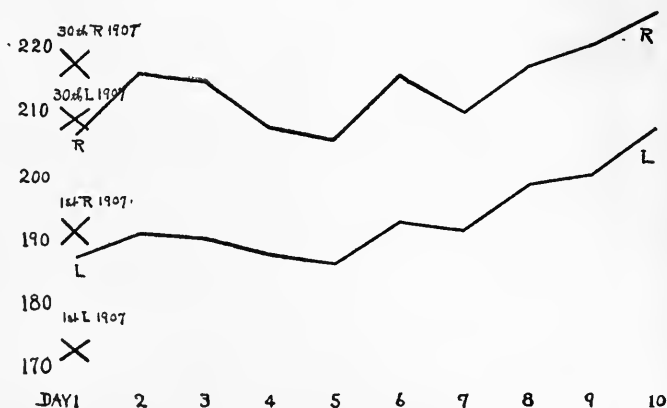
AVERAGE *f*'s SHOWING COMPARATIVE FATIGUE EFFECTS IN THE DIFFERENT 30" SERIES

Series	RIGHT HAND						LEFT HAND					
	Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912		Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912	
1	88.4	2.2	86.1	1.9	87.8	2.0	89.3	1.9	86.5	1.9	85.9	1.5
2	91.0	1.6	86.5	1.7	87.4	1.5	91.2	1.8	89.0	1.4	88.9	1.3
3	88.4	3.5	86.6	1.6	88.2	1.4	89.5	2.7	88.9	1.7	87.3	2.3
4	88.1	1.5	87.5	2.7	88.1	1.0	89.5	2.5	89.3	2.4	87.2	2.0
5	89.3	.9	87.9	1.7	87.5	1.5	88.0	3.2	88.5	1.7	85.5	2.7

What this means is that while the warming up gain of the later series of a day's experiment was in 1907 unequally distributed, being more partial to the final intervals, it is in 1912 much more equally distributed to all the 5" intervals. In 1907 the initial maximum was about the same for all five series of a day, while the endurance became better; in 1912 the initial maximum also increases from series to series, and the other intervals only in proportion to it. This, then, is another acquired property of the test that disuse has altered.

One thinks of practise as most rapid at the beginning; and one would think that on relearning an acquired but partially disused ability the gain would be especially prompt and rapid. How far this is from being the case here, is shown in the following curve:

Taps in 30"



There is no evidence of any coming gain until the 6th day; here there was a 10 days' intermission, that showed nothing abnormal. The gain now continues to the very end, and it should be mentioned that after the ninth day an acute illness caused an intermission that was prolonged to six weeks, before the final experiment.

Sensations of fatigue seem to have been more prominent in the 1912 than in the 1907 experiments. They were at first disagreeably so in the left, less marked in the right, and, as natural, decreased with the renewed practise. Their correspondence on different days with the actual fatigue conditions as indicated by  $f$  is fairly close, and seems to manifest responsiveness by reflex inhibitions to the sensations of fatigue.

The addition and cancellation tests lack the detailed precision of the tapping test, and in the present problem are subject to an additional disadvantage. The practise effect in the tapping test is relatively small, and above all quite slow. Repeated experiments on different days are therefore possible for the correction of chance variations in a single day. In the other two tests, the practise is rapid and great, so that to repeat them in their previous form involves a day to day practise that destroys the relevancy to the problem, of observations extending over different days. The efficiency of only the first day of practise renewal is to be regarded as representative of the loss by disuse. It would probably have been wiser to divide the experimental work so as to extend the tasks of one day over a period of five; this was not done, however, the experiments being repeated for two days in the same form as they had on the last day of previous practise.

The data are more significant than those of the tapping test, as six of the original ten subjects remained available for them; and an interesting point to note incidentally is that while there is everywhere a persistence of the 1910 practise, the nature of the experiment had been sometimes forgotten. A synopsis of the results is as follows:

TABLE V

Experimental Day	(1) Number of additions in 5 minutes on the 1st experimental day, January and March, 1910.	(2) Number of additions in 5 minutes on 2nd experimental day.	(30) Number of additions in 5 minutes on 30th experimental day, February and April, 1910.	(31) Number of additions in 5 minutes on 31st experimental day, December, 1912.	(32) Number of additions in 5 minutes on 32nd experimental day, December, 1912.	(30-1) Absolute gain in number of addi- tions from 1st to 30th days.	(30-31) Absolute loss from February- April, 1910, to December, 1912.	(30+1) Relative gain in number of addi- tions from 1st to 30th days (per cent. which 30th day is of 1st day.)	(30+31) Relative loss from February-April, 1910, to December, 1912.
Average of 6 subjects.	234	274	447	343	375	213	105	191	130
Their M. V. ....	27	28	75	43	53	54	41	22	10
Per Cent. which M.V. is of Average.....	11.5	10.2	16.8	12.5	14.2	25.4	39.1	11.5	7.7
NUMBER-CHECKING TEST. FOR THE WORDS "NUMBER OF ADDITIONS IN 5 MINUTES," ABOVE, read "AVERAGE NUMBER OF O'S CHECKED IN 1 MINUTE, 5 CONSECUTIVE TRIALS."									
Average of 6 subjects.	56 <sup>1</sup>	63 <sup>1</sup>	107	74	80	51	33	192	145
Their M. V. ....	9	7	13	11	10	8	6	13	10
Per Cent. which M.V. is of Average.....	16.1	11	12.2	14.9	12.5	15.7	18.2	6.7	6.9

<sup>1</sup> These figures are the averages of five records made one each on the first and second five experimental days.

In the comparison of losses by disuse we meet here with the same dilemma of the absolute and relative changes. The absolute change appears the better criterion on pragmatic grounds, but both represent real aspects of the change, and both are presented. In the 1910 experiments alone one's interpretation could be somewhat guided by the consideration that the higher a subject is in the scale of practise, the more difficult it is for him to increase his performance by the same absolute amount. In the question of whether superior initial efficiency represented actual practise or practiseability, the results of the 1910 experiments<sup>3</sup> therefore pointed to the latter conclusion. We know that the maximal ability of the 1910 experiments is the result of special practise, but at the outset of the 1912 experiments we have still to discover

<sup>3</sup> Reported in this JOURNAL, XXIII, 1912, 75-88.



whether a person who gains much by practise loses his gain more or less readily than a person who gains but little. We should have perhaps a right to suppose that plasticity will manifest itself in negative as much as in positive directions.

In the addition test, the average drop from the end of the 1910 to the 1912 experiments is 105 units, or not quite half the absolute gain from the beginning to the end of the series of 1910. The mean variation of the 1912 experiments is distinctly less than at the close of 1910. In the absolute amount of practise gain, as well as of later disuse loss, there is much greater relative variation than in any of the daily performances among themselves. The performance at the end of practise is nearly double that at the beginning, and about a third more than that after disuse.

A weak point in the work is that no controls of the quality of vision are available for the different experiments. In one case there is positive report of a better correction of refractive errors in 1912 than obtained in the 1910 experiments. This was in the case of a subject who had shown very slow, though steady, practise effects in 1910; and the somewhat better vision in 1912 would make the loss to 1912 appear less than the fact. However, this subject actually loses on the second day of resumption instead of gaining, and in the number-checking test, the absolute gain here is less than the average.

In the number-checking test, the subjects show almost the same relative practise gain as for the addition test. The tendency to loss by disuse however, is distinctly more, the performance at the end of the 1910 practise being 145% of that at resumption, instead of 130% in the addition test. One subject has lost practically all of the 1910 gain, who had also lost more than any other of the gain in addition.

With the tapping test there is a superficial difference in the situation, for the thirtieth day of 1907 is only 26 taps or 14% ahead of the first one, while the resumption of 1912 shows a loss of 12 taps, the thirtieth day being 106% of this record. However, the disuse loss is thus some 50% of the distance from the end of practise to its beginning; so that while the changes are much less, their proportion is of the same order as the average for the addition and cancellation tests.

These observations introduce two special questions for the data. First, is a subject who gains much by practise also likely to lose much ("easy come, easy go")?

The data do not give a unified answer to this question. In the addition test the situation is fairly clear, large absolute gain going with large absolute loss to a positive correlation

of Pearson 81, that of the relative changes being 58. If the case with relieved eyestrain is excluded, the coefficients become 85 and 49 respectively. In the number-checking test, however, the presence of this case alters to .56 a coefficient that is otherwise -.20, while in the relative changes a negative coefficient of -.33 results from this case and that previously mentioned who lost all the gain, which was relatively little. If the former case is excluded, the coefficient is -.19, if both, it is distinctly plus. It is by no means uninteresting to observe these extreme departures from the supposedly normal tendency. The probable meaning of the results is that while, when the practise curve follows a normal form, a loss by disuse may indeed be expected in proportion to the amount of practise gain, "irrelevancies" bring about anomalous forms of the practise curve in special cases, where such relationship does not appear.

A further question concerns the likelihood of a subject who loses much in one function to also lose much in the other. As measured by the Pearson coefficient, this seems to be strongly the case, more so with the absolute losses than the relative ones. In this connection it is noteworthy that if one compares the ground covered between the beginning and end of practise in the 1910 experiments, one finds a negative correlation in the improbability of the two tests, which in the relative gains is extreme. Other data do not show this negative relationship so consistently, and it is probably a good deal slighter than appears here. Still, the interesting result is reached that disuse loss is a more generalized property of the organism than practise gain. The capacity to acquire may be high for some abilities and low in others, but the tendency to forget them seems to be more equal for different abilities. The figures illustrating this point are,

Correlation between		
Addition test and number checking test,		<i>r</i>
	absolute practise gain	-30
	absolute disuse loss	78
	relative practise gain	-95
	relative disuse loss	64

In sum, such observations emphasize the relativity of certain knowledge, and of memory. Pragmatically, to know a thing is to have established an association or co-ordination path which causes us to react according to it. We know a thing more certainly according as we react more certainly or in any way more effectively as though that thing were so. Though the certainty has a different source, you do not know that

two and two make four more certainly than the dervish knows the prophet's paradise awaits him on death in battle for his faith. To say that one takes a few sigma longer to add six and five than one did two years ago, is another way of saying that he does not know their sum so well; and this same lessened decisiveness of response, many times magnified, finally produces false reactions as in the method of right and wrong cases. We lose the knowledge that six and five make eleven just as we lose the knowledge of how to effectively return a service at tennis, or of how to make 230 taps in 30 seconds. Our tendency to lose in these accomplishments also appears more generalized than do our abilities to acquire them. Another feature of this sort of deterioration is its relative independence of positive interference from without, for no considerations enter to make what were the proper responses before, not the proper ones now. In this respect the work falls short, as most of experimental psychology does, of meeting the conditions of actual life, where success is a matter not simply of knowing, but of knowing the right things.

## ON PSYCHOLOGY AS SCIENCE OF SELVES

By JOSEPHINE NASH CURTIS<sup>1</sup>

### THE ADVANCING CLAIMS OF SELF-PSYCHOLOGY

Self-psychology was at first brought forward by Professor Calkins as one of the possible points of view in psychology. In "Psychology as Science of Selves" (1900) she writes: "This essay [insists] . . . on the equal validity of two underlying forms of psychology . . . . The first of them may perhaps be named 'Atomistic Psychology'. It treats of . . . psychic phenomena, considered without reference to any self, and its primary procedure is the analysis of these psychic facts into irreducible elements and the classification of complex phenomena according to the preponderance of given elements. For the second of these ultimate forms of psychology I have vainly sought a satisfactory name. It is distinct from the science of the bare psychic contents, and has been variously regarded as a study of conscious functions, of mental operations, and of activities of the self. Most simply and with adequate recognition of the profoundly social nature of the self, this form of psychology may be treated as the science of conscious selves."<sup>2</sup>

In the "Introduction to Psychology" (1904) we find similar statements. "The book is written in the conviction that psychology should study consciousness, both as a series of complex mental processes, or ideas, and as a relation of conscious selves to each other"<sup>3</sup>; and, again, "We defined science as the study of facts or phenomena, that is of limited bits of reality, taken for granted without investigation of their relation to the whole of reality. Now it is certain that consciousness, or ideas, regarded without reference to a conscious self, may form the material of a scientific psychology; and some psychologists have limited the science to the study of these momentary contents of consciousness, not regarded as the experiences of a self. But it is equally evident, in the opinion of the writer,

<sup>1</sup> From the Psychological Seminary of Cornell University.

<sup>2</sup> M. W. Calkins, *Psychology as Science of Selves*, *Phil. Rev.*, 9, 1900, 490.

<sup>3</sup> *An Introduction to Psychology*, 1904, v.

that selves also may be treated as facts or phenomena, because they are taken for granted by everyday people, without inquiry about their relation to 'reality.'"<sup>4</sup>

In "Der doppelte Standpunkt in der Psychologie" (1905) Miss Calkins again urges her position. "Psychologically we may consider consciousness from two points of view: we may conceive consciousness either as a series of linked psychical processes, wholly without reference to the conscious I, or as a many-sided consciousness of the real I in its relations. In accordance with these two points of view we have a twofold psychology: process-psychology and I-psychology. Every consciousness can be looked at from both points of view: in other words, both methods can be consistently worked out."<sup>5</sup>

So far (1905) the point of view of self-psychology has been urged on the ground that it is as valid a form of psychology as 'atomistic' (or 'structural') psychology. Now, however, we find it offered as a mediator, a reconciliation between other forms of psychology. In "A Reconciliation Between Structural and Functional Psychology" (1906) Miss Calkins writes: "I shall . . . try to show [that] that psychologist's self is rightly conceived as fundamental both to structural and to functional psychology, and that it should therefore be studied by both methods."<sup>6</sup> The conception of the self, we read, "facilitates, and necessitates a union between structural and functional psychology."<sup>7</sup> The argument is that: "(1) structural psychologists and one group (but one group only) of functional psychologists are unjustified in their doctrines of the basal psychic phenomenon; but that (2) in their doctrines of psychological analysis, both structural and functional psychology are right: the analysis of the one supplements that of the other . . . (3) . . . both sorts of analysis, structural and functional, are essential to an adequate self-psychology." In greater detail, Miss Calkins argues as follows: "Structural psychology consists essentially in the teaching that the task of psychology is first, to analyse typical experiences until one reach irreducible elements, and second, to classify the ordinary sorts of complex experience according as one or another of these elements predominate . . . Now it is past doubt that this structural analysis of a psychic state is always possible . . . But this analysis into structural elements—it must be insisted—is not necessarily the analysis of an idea or psychic

<sup>4</sup> *Op. cit.*, 156.

<sup>5</sup> *Der doppelte Standpunkt in der Psychologie*, 1905, 32 f.

<sup>6</sup> *Psych. Rev.*, 13, 1906, 61 ff.

<sup>7</sup> *Op. cit.*, 68.

event: it is, on the other hand, quite as easy to discover the structural elements regarded as experience of a self, for though every conscious experience is some relation simple or complex, of a self to its environment, it is also a complex of different elemental experiences, sensational, affective and the like . . . . In a word, psychology as science of conscious and related selves may and should undertake the analytic discovery of elements of conscious experiences as such. By virtue of this structural procedure it is truly a structural psychology, though it utterly repudiates the doctrine of the psychic event, or idea as the basal fact of psychology." After some discussion Miss Calkins concludes that "functional psychology, rightly conceived, is a form of self-psychology, that its basal phenomenon is the psychologist's self, and that its significant contributions to psychology are, first, its doctrine of the inherent relatedness of self to environment, and second, its insistence on the progressive efficiency or utility of these relations . . . . Pruned of . . . biological excrescences, a functional psychology . . . is a self-psychology."<sup>8</sup>

Self-psychology, then, has been offered first as a possible form of psychology; and next as the form which "harmonizes the essential doctrines of a structural and of a functional psychology."<sup>9</sup> As we might expect, out of the latter conception comes the belief that self-psychology is a form of psychology superior to the others. This idea is developed to some extent in the article just cited. Miss Calkins says: "I shall aim to show . . . first that these actual (concrete conscious) experiences cannot adequately be described by enumerating their structural elements, and second, that the conception of them as relations of self to environment involves or allows all the teachings essential to functional psychology." In a later article<sup>10</sup> is a more insistent statement: "I am myself profoundly convinced that consciousness is never adequately conceived except as conscious self; and that both the other conceptions of consciousness [structural and functional] imply this." The inadequacy of structural psychology is stated as follows: "If I conceive psychology as science of ideas I inevitably raise the scientifically relevant question:<sup>11</sup> Whose idea? and then I

<sup>8</sup> *Op. cit.*, 75.

<sup>9</sup> *Op. cit.*, 76.

<sup>10</sup> *Journ. of Phil., etc.*, 4, 1907, 677 f.

<sup>11</sup> It may be suggested here that Miss Calkins must be generalizing from her own experience. Many psychologists have conceived psychology as "science of ideas" without raising the question; indeed, these psychologists see no more necessity for raising it than the physiologist sees for asking "Whose muscle?" or the geologist "Whose stone?"

arbitrarily refuse to answer my own question. In other words, the 'idea' is immediately experienced as idea of a self, or subject, mind, ego—call it as one will. To refuse to deal with this self is indeed theoretically possible, but is a needlessly abstract, an artificial, an incomplete procedure."<sup>12</sup> Moreover, "an idea psychology is incapable, through this fundamental inadequacy, of describing some, at least, of the facts which it studies." Functional psychology, also, is inadequate. We find that "narrowly scrutinized, the theory of psychology as science of mental activities turn out to be a needlessly abstract, an arbitrarily inadequate view. For activity is clearly a character of something . . . One inevitably and rightly asks concerning any character not only 'of what sort is it?' but 'whose is it?' . . . In truth, the conception of mental activity requires the conception of mental actor, even more obviously than the full conception of the idea includes that of its possessor."<sup>13</sup>

The superiority of self-psychology is still further set forth in Miss Calkins' "First Book in Psychology" (1910). In the preface we read: "This book has been written in the ever strengthening conviction that psychology is most naturally, consistently, and effectively treated as a study of conscious selves in relation to other selves and to external objects—in a word, to their environment, personal and impersonal . . . I have . . . abandoned . . . [the] double treatment [of psychology both as science of selves and as science of ideas] . . . not because I doubt the validity of psychology as study of ideas, but because I question the significance and the adequacy, and deprecate the abstractness of the science thus conceived."

In the last edition (1914) of this book we find many indications not only that self-psychology is more consistent than other forms of psychology, but also that the change in point of view has rendered former expressions inadequate. One example of this occurs in the preface.<sup>14</sup> "If I were writing this book *de novo* I should throughout refer to the sensational, affective, and reflective elements in such a way as to emphasize the fact that they are found through analysis of consciousness conceived as relation of self to object. Thus I should use the expressions 'seeing colors,' 'hearing tone-qualities,' 'liking,' 'distinguishing,' along with, or even in place of the parallel expressions, 'visual and auditory qualities' . . ."

<sup>12</sup> *Op. cit.*, 678 f.

<sup>13</sup> *Journ. of Phil., etc.*, 682.

<sup>14</sup> xii f.

We have now outlined in brief the advancing claims of self-psychology, first as a possible, then as a reconciling, and last as the most natural, consistent and effective form of psychology. With this introduction to the subject, we shall consider the system of self-psychology; and shall discuss in order its subject-matter, method, problem, and results; for it is with reference to these points that any scientific system must be appraised. The subject-matter of self-psychology is, of course, the self; and it is to the self-psychologist's self that we shall first turn our attention.

## THE SYSTEM OF SELF-PSYCHOLOGY

### *The Concept of the Self*

Miss Calkins' conception of the self is not clear. In the most general way, her self is a fact, a phenomenon, that stands at the same level as qualities, things, moments, events. The self is also conceived as having characters. Although Miss Calkins follows her general statements immediately with her discussion of the characters, for the sake of clearness we shall first treat of the general concept and then, in another section, of the characters.

The self is a fact, in the same category with things and events. It may be viewed in the light of various metaphysical systems, but "no one of these . . . systems precludes the scientific treatment of selves."<sup>15</sup> These statements, of course, tell us nothing definite. A further search reveals that "besides realizing my conscious experiences, or feelings, I am also conscious of my conscious self, as in a sense including, but not as identical with, the perceptions, the emotions, or the thoughts of any given moment;"<sup>16</sup> and in "*Der doppelte Standpunkt*" we read: "Self-consciousness can—at least partially—be described from two sides. We can first analyse the self-consciousness as well as the psychical processes into so-called elements."<sup>17</sup> Miss Calkins recognizes three sorts of "structurally elemental consciousness:"<sup>18</sup> sensational, affective, and relational. But in what sense, we may ask, does my conscious self include the perceptions of the moment? It is evident that, for Miss Calkins, the self is not merely the sum of its perceptions, emotions, and the like; but we are not told what it is in addition to these, or what it is instead of the sum-total of these. She adds, however, that "with the exception of the

<sup>15</sup> *Phil. Rev.*, 492.

<sup>16</sup> *Introduction to Psychology*, 151 f.

<sup>17</sup> 34 f.

<sup>18</sup> *First Book*, 1914.



analysis into structural elements, the only description of self-consciousness is, first, as consciousness of myself contrasted with other selves, and second, as consciousness of my varying relations or attitude to these other selves."<sup>19</sup> Unfortunately, this statement does not aid us; for instead of continuing the line of thought and telling us the differences between ourself and other selves, she considers only the consciousness of "my varying relations to other selves." As we are not, at present, interested in the relations of the self, but merely in the self itself, we must continue the search for information concerning it.

In "Der doppelte Standpunkt" we read: "This I, the self or subject, cannot, of course, be defined, for it is the most intimate, most fundamental thing that we know, and, on that account cannot be reduced to other terms. The I is simply the I; everyone knows for himself what it is . . . We can first analyse the self-consciousness . . . into so-called elements . . . In the second place, we know that the real self or I is known in contrast and in relation to other selves; so we reach the important conclusion that each I is essentially a social I."<sup>20</sup> There are various comments to be made on this passage. In the first place we must acknowledge that, if it is true that the self cannot be defined, we are unreasonable in expecting Miss Calkins to define it. But as she distinguishes between selves and "things," she should be able to point out the differences (attributive or other) between the two. In the second place, it is surely not true that everyone knows for himself what the I is. The actions of the plain man may be *interpreted* to mean that he knows what the I is,<sup>21</sup> just as his choice of language in speaking of the sun might be interpreted to mean that he knows the sun revolves about the earth. Yet astronomy would be no science if it took as its fundamental fact that the sun revolves about the earth, just because everybody says that it does. What answer can Miss Calkins make to the person who says: "I do not know what the I is?" In the third place, it is no assistance to one hunting for a description of the self to say: "the real self is *known*

<sup>19</sup> *Introduction to Psychology*, 151 f.

<sup>20</sup> 34 f.

<sup>21</sup> The writer took occasion recently to ask a number of persons, none of them students of psychology, exactly what they meant by the I. The answers were marked by variety and evasiveness; and the majority might be simmered down to the one given by an intelligent working-man after a day's meditation. This was the answer: "Conundrum, ain't it?" The offhand appeal to the 'plain man' of the philosophers is, indeed, a curious recourse for the scientific psychologist. Why not collect observations?

in contrast and in relation to other selves." Aside from the circularity of the statement, it is no *description* to say the self is *known as related*. *What* it is that is known, how it is known as related, and under what conditions it is known as related, are not stated. And, for one, the writer must assert that either she does not understand the expression or else she does not know herself as related, except that as the result of a process of logical reasoning she may conclude that she must be related.

In "A Reconciliation Between Structural and Functional Psychology," Miss Calkins writes: "By self as psychic fact I mean what the plain man means by self, in so far as this does not involve the view that body constitutes part of a self. This conscious self, the plain man's self, in the developed form in which we commonly study it, is in the first place realized as underlying the experiences of the moment—as having percepts, images, and the like, or more exactly, as 'perceiving,' 'imagining,' and 'feeling.' And, in the second place, every self, besides being fundamental to its own ideas or experiences, is also a related self. That is to say, I am always conscious of myself as in some way related to my environment social or physical."<sup>22</sup> It is undoubtedly true that the "plain man's self," if it be realized at all, may be "realized as underlying the experiences of the moment" and as being related to environment. But in that case is not the "plain man's self" decidedly metaphysical in character? Is not what he considers the "self of selves" the part which he expects to live, his immortal soul? There must be some foundation for the expression, so common in everyday life, "trying to keep soul and body together." Taken at its face value, the expression implies that a self has two parts: body and soul. Indeed, the other common expression "mind, body, and soul" and the corresponding "physically, mentally, and spiritually" imply a three-part self. Miss Calkins has intentionally omitted the body from her "plain man's self;" but she has also omitted the soul, or spirit, the part of the self which the "plain man" apparently considers the most important.

Let us sum up what we have learned so far about the nature of the self. (1) The concept of the self, as given to us by Miss Calkins, is far from clear; (2) beside structural analysis we must describe the self in terms of the difference between it and other selves—without having these differences stated; (3) the self cannot be defined; (4) by self Miss Calkins means the plain man's self, but she has deprived him of his body and soul.

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<sup>22</sup> *Psych. Rev.*, 13, 1906, 63 f.

All these general statements concerning the self really tell us nothing. But before we go on to consider the characters which Miss Calkins ascribes to the self, we shall do well to examine what seems to be the proximate source of this self, and see if that clears the concept at all.

### *James' Concept of the Self*

In his "Briefer Course" James gives the fundamental thought of self-psychology. "Whatever I may be thinking of, I am always at the same time more or less aware of *myself*, of my *personal existence*. At the same time it is *I* who am aware."<sup>23</sup> In the "Principles of Psychology" we read such passages as: "The universal conscious fact is not 'feelings and thought exist' but 'I think' and 'I feel';"<sup>24</sup> "Our own bodily position, attitude, condition, is one of the things of which *some* awareness, however inattentive, invariably accompanies the knowledge of whatever else we know. We think and as we think we feel our bodies as the seat of the thinking . . . *Whatever* the content of the ego may be, it is habitually felt *with* everything else by us humans;"<sup>25</sup> "All people unhesitatingly believe that they feel themselves thinking . . . I regard this as the most fundamental of all the postulates of psychology."<sup>26</sup> When we ask what this self of James' really is, we find that, in the widest possible sense, it is the sum total of all a man can call his; "not only his body and his psychic powers, but his clothes and his house, his wife and children, his ancestors and friends, his reputation and works."<sup>27</sup> We find also that there are various constituents of the self, which may be divided into two classes: the Empirical self, made up of the Material Self, the Social Self, and the Spiritual Self, and the Pure Ego. The characters of these selves are given in some detail: "The body is the innermost part of the Material Self in each of us . . . The clothes . . . Our immediate family . . . Our home . . . A man's Social Self is the recognition he gets from his mates . . . Properly speaking a man has as many social selves as there are individuals who recognize him and carry an image of him in their mind . . . A man's fame, good or bad, and his honor or dishonor, are names for one of his social selves . . . By the Spiritual Self, so far as it belongs to the Empirical Me, I mean a man's inner or sub-

<sup>23</sup> W. James, *Psychology*, 1905, 176.

<sup>24</sup> W. James, *The Principles of Psychology*, 1890, I, 226.

<sup>25</sup> *Op. cit.*, 241.

<sup>26</sup> *Op. cit.*, 185.

<sup>27</sup> *Op. cit.*, 291.

jective being, his psychic faculties or dispositions, taken concretely; not the bare principle of personal Unity, or 'pure' Ego . . . These . . . are the most enduring and intimate part of the self."<sup>28</sup> James then goes on to discuss this "self of all the other selves," and concludes that everybody would call it the "active element in all consciousness." "This central part of the self is . . . no *mere* summation of memories or *mere* sound of a word in our ears. It is something with which we also have direct sensible acquaintance." Upon careful examination, this "self of selves" proves for James to consist mainly of "peculiar motions in the head or between the head and throat."

We find, then, in James the fundamental thought of self-psychology: that "I am always more or less aware of myself." A corresponding statement is made by Miss Calkins: "I am always, inattentively or attentively, conscious of the private, personal object, myself, whatever the other objects of my consciousness."<sup>29</sup> On the face of them, these two statements seem almost identical; but in reality they are quite different. The self of which James was "always more or less aware" was mainly, motions in the head; that of which Miss Calkins is "always . . . conscious" cannot be defined. James' self is carefully differentiated from the other selves, the selves which seem important to the plain man; Miss Calkins' self is the plain man's self (*minus* body). We find in James a detailed discussion of what we feel when we become aware of the existence of the 'self of selves'; we find no such detailed discussion in Miss Calkins, but are told only that the self is "immediately experienced as possessed of characters."

It is evident that the fact that James gives us the fundamental thought of self-psychology does not make James a self-psychologist. In the first place, we find in James several exceptions to the ever-present awareness of self. He says: "A mind which has become conscious of its own cognitive function, plays . . . 'the psychologist' upon itself. It not only knows the things that appear before it; it knows that it knows them. This stage of reflective condition is, more or less explicitly, our habitual adult state of mind. It cannot, however, be regarded as primitive. The consciousness of objects must come first. We seem to lapse into this primordial condition when consciousness is reduced to a minimum by the inhalation of anaesthetics or during a faint."<sup>30</sup> He then quotes

<sup>28</sup> *Op. cit.*, 292 ff.

<sup>29</sup> *First Book*, 1914, 5.

<sup>30</sup> *Principles*, 272.

Herzen to the effect that "at the beginning of coming-to, one has at a certain moment a vague, limitless, infinite feeling—a sense of *existence in general* without the least trace of distinction between the me and the not-me," and gives various other instances in which no consciousness of self is present. The statement that I am always "more or less aware of myself" must be changed for James to read "The normal human adult under normal conditions is always more or less aware of himself." Self-psychology, on the other hand, is founded on the postulate that the statement is *always* true for man, and is true for babies and animals as far as they are conscious at all.<sup>31</sup> Again James seems to disagree with the first principles of self-psychology when he says: "Many philosophers . . . hold that the reflective consciousness of the self is essential to the cognitive function of thought. They hold that a thought, in order to know a thing at all, must expressly distinguish between the thing and its own self. This is a perfectly wanton assumption, and not the faintest shadow of reason exists for supposing it to be true. As well might I contend that I cannot dream without dreaming that I dream, swear without swearing that I swear, deny without denying that I deny, as maintain that I cannot know without knowing that I know. I may have either acquaintance-with, or knowledge-about an object *O* without thinking about myself at all. It suffices for this that I think *O* and that *O* exists. If, in addition to thinking *O*, I also think that I exist and that I know *O*, well and good."<sup>32</sup> But Miss Calkins says that "to be conscious is to be conscious of my conscious self." A third difference between James and the self-psychologist is that James "notices" the characters of thought, whereas Miss Calkins "immediately experiences" the characters of the self. We need not stop here to discuss this point, as the question of the immediate experiencing of the characters of the self will be considered in a later section of the paper. The fourth difference between James' and Miss Calkins' selves has already been suggested: the fact that James' self is described in terms of sensation, while Miss Calkins' self cannot be expressed in terms either of idea or of function, but is immediately experienced as having certain characters.

Our consultation of James has brought out the fact that, although he gives the fundamental thought of self-psychology, the self of which he speaks is not the self of which Miss Calkins speaks. If we try to equate Miss Calkins' self with one of James', we find, first, that her self cannot be the Material

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<sup>31</sup> *Journ. of Phil.*, etc., 5, 1908, 68.

<sup>32</sup> *Principles*, I, 274.

Self and cannot be the Social Self; neither can it be the pure Ego. If, then, it is any of James' selves, it must be the Spiritual Self. At first glance, it seems to resemble this self; only that James discovers "motions in the head," while Miss Calkins comes out with nothing of the sort. We must, therefore, conclude that James, although he gives us a perfectly clear idea of his self, helps us very little toward understanding Miss Calkins' self.

### *The Characters of the Self*<sup>33</sup>

It is hardly fair to Miss Calkins, however, to give up the search for the real nature of the self until we have examined her account of its characters; for, as she says, we are not merely always conscious of ourselves, but we are also conscious of ourselves as possessing certain characters. These characters, moreover, are said *not* to belong to idea or to function,<sup>34</sup> and so are employed to distinguish the self from idea and function. In the "First Book"<sup>35</sup> we read: "The conscious self . . . is immediately experienced as possessed of at least four fundamental characters. I immediately experience myself as (1) relatively persistent . . . as (2) complex . . . as (3) a unique, irreplaceable self . . . I experience myself as (4) related to (or, conscious of) objects either personal or impersonal."<sup>36</sup>

### *The Persistence of the Self*

The first character of which Miss Calkins treats is persistence. The self, we read, is experienced as "in some degree," "in some sense," "relatively" persistent; "in other words, I am in some sense the same as my childhood self."<sup>37</sup> By persistence

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<sup>33</sup> The characters of the self are discussed by Miss Calkins in *An Introduction to Psychology*; *Der doppelte Standpunkt in der Psychologie*; *Journ. of Phil.*, etc., 5, 1908, 64 ff.; and *First Book in Psychology*.

<sup>34</sup> *Journ. of Phil.*, etc., 65.

<sup>35</sup> 1914, 3.

<sup>36</sup> There is a passage in James (I., 225) which seems to suggest, at least, the characters which Miss Calkins ascribes to the self. "How does [thought] go on? We notice immediately five important characters in the process. . . . (1) Every thought tends to be part of a personal consciousness. (2) Within each personal consciousness thought is always changing. (3) Within each personal consciousness thought is sensibly continuous. (4) It always appears to deal with objects independent of itself. (5) It is interested in some parts of these objects to the exclusion of others." Here certainly may be found the germs of persistence, complexity, and relatedness.

<sup>37</sup> *First Book*, 3.

is not meant ultimate self-identity, but the "kind of identity of which one is immediately conscious."<sup>38</sup>

What precisely do the above statements mean? A translation of the sentence "I am conscious of myself as relatively persistent" might read "I, the present self, am continually conscious of a past self." I, in a sense, *i. e.* in so far as I go by the same name, in so far as I have some of the same ideas, habits, relationships to friends, in general in so far as I have the same functional relations to my environment that I formerly had, am still the same I. But is it not just as true that, in a sense, *i. e.*, in so far as I have different functional relations to my environment, behave differently in the face of the same objective situations, have different ideas, different habits, and so on, I am a different I? Do I not, in other words, just as often, just as emphatically, and just as immediately experience myself as different from my childhood self? And if the first consideration means that self is persistent, does not the second mean that self is at the same time, or in another sense, just as non-persistent as it is persistent? 'Relatively persistent' surely means 'not exclusively or absolutely persistent.' The positive logically involves the negative statement; partial sameness necessarily involves partial difference. Non-persistence, therefore, has the same claim to be a character of the self as has persistence.

But not only may we question whether persistence has a better claim than non-persistence to be considered a character of the self; we may also ask if persistence is *exclusively* a character of self. Miss Calkins dismisses Angell's statement that "general functions like memory (are) persistent" with the remark that "if one scrutinize the real meaning of this statement 'memory—or reason—or will is a persistent function' one finds it to be simply this, that one and the same self at any time may remember or reason or will."<sup>39</sup> Why does she not say that it means that one and the same self *at all times does remember and reason and will*? That surely seems to be the meaning which Angell intends. Mind for the functional psychologist is a general function (for example, accommodation to the novel) within which less inclusive but still quite general functions (memory, thought, etc.) may be discriminated. Never in the concrete do we find any single general function active in isolation; but, wherever we find consciousness, there we find all these general functions; and in so far

<sup>38</sup> *Journ. of Phil., etc., loc. cit.*

<sup>39</sup> *Ibid.*

as consciousness is persistent, in *so far* are the functions persistent.

Persistence, then, is no more a character of the self than is non-persistence, nor is self to be distinguished from function by means of this character. On the contrary, self appears to be merely another name of the all-inclusive mental function.

### *The Uniqueness of the Self*

We come now to the question of the uniqueness of the self, and of the relation of this uniqueness to the persistence of the self. We find that "I immediately experience myself . . . as a unique, irreplaceable self—I am closely like father, brother, or friend, but I am, after all, only myself; there is only one of me."<sup>40</sup> "This uniqueness is . . . experienced most clearly in our emotional and volitional consciousness: when we reflect upon it we may describe it as a consciousness of a this-which-could-not-be-replaced-by-another. Now we simply are not conscious of ideas and functions as, in this sense, unique. A given self, with a different idea, is still this self; whereas a given idea is this or that idea according as it belongs to this or that self. I am I whether I see or hear, whether I fear or hope, but another self's vision or fear, however similar, is not this experience, but another."<sup>41</sup>

But *why*, we may ask, is a self with a different idea still the same self? Miss Calkins, herself, as we shall see (p. 82), insists that one essential, never-to-be-neglected character of the self is the having, among other things, of ideas. Is it, then, that the *having* is the same whatever the idea is that is had? Is it the same thing to have one idea as to have another? May there be a having without an idea had? May we, so far as self is concerned, abstract from idea but not from the *having of ideas*? Is a self without any ideas possible? Are we in dreamless sleep, *e. g.*, still self-conscious? These and other questions, for which we find no answer, are at once suggested. Certainly, the concept of having and the other concepts so freely used need clarification and definition. Certainly, if (as Miss Calkins maintains in another place) "there can be no objection to considering ideas as part of the self,"<sup>42</sup> then a change of a part means a change of the whole, and the self with another idea is not, and cannot be regarded as, the same self.

"A given idea," says Miss Calkins, "is this or that idea

<sup>40</sup> *First Book*, 3.

<sup>41</sup> *Journ. of Phil.*, etc., 66.

<sup>42</sup> *Ibid.*



according as it belongs to this or that self." In the sense of a structural psychology,—and it is in this sense that Miss Calkins on her own assertion speaks of ideas,—the essence of ideas does not consist in their being had by a self, but rather in their attributive determinations. If ideas differ *only* in respect to what self has them, then all the ideas of a given self are identical, or a given self can have only one idea. In fact, ideas, in the sense both of structural psychology and of the passages of Hume which Miss Calkins quotes in her own behalf in another connection, are themselves unique. "Another self's vision or fear, however similar, is not this experience, but another," says Miss Calkins. True, but not the whole tale! In exactly the same way, *my present* vision or fear, however similar to my *past* vision or fear, is not my past vision or fear, but another. That is Hume's story. And what holds of the uniqueness of ideas holds also of functions. Why is not a remembering-now a not-to-be-replaced-by-another remembering? I can never remember at this particular now again; I never did remember at this particular now before. But the case is not yet ended; uniqueness is not peculiar to ideas and functions. A given chair is unique, every hair on my head is unique; they 'cannot be replaced by others.' In the most general way, every concrete object, every actually existing thing in the world is unique.

And the question now arises, can a self be unique and not persistent, or persistent and not unique? The plain man probably considers himself unique. But he does not talk as if he considered himself persistent when he says, for example, of himself or another, that he "lost himself," "forgot himself," "could not recognize himself," "was not himself," "was out of himself."<sup>43</sup> These expressions sound, at any rate, as if the plain man's self were for him an ideal and not a real persistent being. Moreover, there are times when we become so absorbed in a book or play that for the time being we, to all intents and purposes, *are* the hero, say, and apparently have no consciousness of our ordinary self. Granted that in such a case we are unique; are we also persistent? If we are persistent because we are in some sense the same as the childhood self of the hero whom we for the moment are, then we must be any number of persistent selves. But this cannot be; for we are, by hypothesis, unique: "I am closely like father, brother, or friend, but I am, after all, only myself; there is only one of me." But if I can be Hamlet, why can I not at some other time be "father, brother, or friend?" Or, in

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<sup>43</sup> E. B. Titchener, *Text-book*, 1910, 13.

general: is this self a this-not-to-be-replaced-by-another, if we are at various times various selves? Miss Calkins has answered the question of "losing oneself" in a book or play by saying: "Selfconsciousness is surely present when a man seems to himself 'a person' even though another person. He is unconscious . . . of circumstances, of surroundings, of the past, but not of self."<sup>44</sup> But he cannot be conscious of self (according to Miss Calkins) without being conscious of that self as persistent, complex, unique, and related. If he is conscious of himself *in this situation* as persistent and unique, then surely he may have as many selves as he desires, or has thrust upon him, each one of them unique and relatively persistent; a curious condition for the plain man's self!

Uniqueness, then, cannot be a distinguishing character of the self, because ideas and functions and all concrete objects are also unique. In the second place, it is difficult to apply the characters of uniqueness and persistence together to all the situations in which the self is found.

#### *The Complexity of the Self*

A third character of the self is that it is "inclusive; it is, in other words, a complex of ideas, functions, experiences."<sup>45</sup> "I am a perceiving, remembering, feeling, willing self."<sup>46</sup> "This is the character of the self which gives to the idea and function conceptions of psychology their hold on psychological thought; for against ideas or functions regarded as parts, or aspects, of the self no crucial objection need be urged."<sup>47</sup>

Whatever the concept of the self, there can be no doubt that complexity is one of its characters. But Miss Calkins has neglected, in the case of complexity, to prove the thesis with which she started: namely, that the characters of the self do not belong to idea or function. She makes no attempt to prove that ideas and functions are not complex; and it is evident that no such proof could be offered, for, in this sense, we have no simple experiences. Complexity, therefore, cannot be used as a distinguishing character of the self.

#### *The Relatedness of the Self*

We come to the fourth and last character of the self, its relatedness. "I immediately experience myself . . . as related to (or, conscious of) objects either personal or impersonal. For example, I am fond of my mother . . . and I am tasting

<sup>44</sup> *Psych. Bull.*, 9, 1912, 25 ff.

<sup>45</sup> *Journ. of Phil.*, etc., *loc. cit.*

<sup>46</sup> *First Book*, *loc. cit.*

<sup>47</sup> *Journ. of Phil.*, etc., *loc. cit.*

an orange . . . It would be impossible to enumerate all the 'personal attitudes' or relations of self to its objects. The following, however, are fundamental . . . receptivity and activity; sympathy . . . attention."<sup>48</sup> This character is the keynote of Miss Calkins' whole system of psychology; for we find that the full definition of psychology is "the science of self in relation to environment"<sup>49</sup> or the "science of the self in relation to, or conscious of, its environment."<sup>50</sup>

In the "Introduction to Psychology" we have: "I, who read this paragraph . . . simply cannot be conscious of my own self except as related in the most varying ways to a vast number of other people. Let one try to drop out of the consciousness of oneself the realization, however vague, of some or all of these relations, the consciousness that one is son, brother, member of a fraternity, student at a university, citizen of the United States: such an imagined elimination of the consciousness of his social relationships leaves a man, in truth, with nothing which he can recognize as himself." In the 1914 edition of the "First Book" Miss Calkins states that she uses the term consciousness "as synonym for 'personal attitude' or 'the self's relatedness to its objects.'"<sup>51</sup> Now, if one define consciousness as relatedness to environment, then it must of course be true that one is conscious of all of one's relations. So if we accept Miss Calkins' definition of consciousness,<sup>52</sup> we must accept her statement as to the relatedness of the self. But it is curious to "immediately experience" four characters of the self, and then to find that one of these characters really includes the other three. Yet, under the above definitions, this is evidently the case with the character of relatedness. The self must be "conscious of" the persistence, the complexity, and the uniqueness of the self; how else can these characters be known? But to be conscious of means to be related to, and so we come back to the fourth character. What are the persistence, uniqueness, and complexity of the self other than ways in which the self may be *related*? Let us first consider persistence. Why should not the character of persistence be included under that of relatedness? By 'rela-

<sup>48</sup> *First Book*, 1914, 3.

<sup>49</sup> *First Book*, 1910, 273.

<sup>50</sup> *First Book*, 1914, 1.

<sup>51</sup> xii f.

<sup>52</sup> This definition of consciousness, when considered with reference to the character of relatedness, presents certain difficulties: for example, if we try to substitute words which Miss Calkins uses as synonyms we become confused. The sense of the following is scarcely clear: "I am related to myself as related to objects."

tively persistent' I mean "I am in some sense the same as my childhood self." Does that not also mean that "I experience myself as in some way related to my childhood self?" In the second place, how can I experience myself as unique, as "closely like father, brother, . . . but after all, only myself," without differentiating myself from father and brother? But I surely cannot differentiate myself from others without experiencing myself as in some way related to them. And as for the complex character of the self, I cannot again experience myself as "perceiving, remembering, feeling, willing self" without experiencing myself as perceiving, etc., something, that is, as *related to* an object, personal or impersonal. But we need not continue, since Miss Calkins herself writes:<sup>53</sup> "The relatedness of a self . . . may be shown to be a character of all consciousness."<sup>54</sup>

We find, further, apparent inconsistencies in the treatment of the different characters. For example, in the 2nd edition of the "First Book" the structural elements are assigned to the complex character: "The mental reduction of this complex experience to its lowest terms gives what are called the structural elements of consciousness;"<sup>55</sup> while in the 4th edition they are assigned to the character of relatedness: "It is possible by attentive introspection to detect within every . . . relation of the self to object, certain elemental (that is, further irreducible) factors or constituents . . . the 'structural elements' of consciousness," and: "From this follows the conception of the sensational, affective and relational 'elements' . . . as constituents of all forms of the relatedness of self to objects."<sup>56</sup> In spite of such statements as the last two, Miss Calkins makes complexity a separate character of the self, and further defines it by saying "I am a perceiving, remembering, feeling, willing self." But surely the 'structural elements' come from the perceptions, etc.; so that they must come from both complexity and relatedness. This result reinforces our former conclusion that the character of relatedness really includes the other three characters.

We have now considered the four characters of the self; have shown that persistence is no more a character of the self than non-persistence is; have shown that some functions at least are persistent; that ideas and functions as well as selves

<sup>53</sup> This statement reads as if persistence, uniqueness, and complexity were *not* characters of all consciousness, as they have at other times been stated to be.

<sup>54</sup> *Phil. Rev.*, 17, 1908, 272 ff.

<sup>55</sup> 14.

<sup>56</sup> xii.

are unique, and that therefore the character is not a distinguishing one; that a self cannot always be both unique and not non-persistent; that ideas and functions as well as the self are complex; that, given Miss Calkins' definition of consciousness, the self must be acknowledged as related; but that, under the definitions of the various characters, persistence, uniqueness, and complexity must be accepted as forms or modes of the fourth character, the relatedness.

The conclusion is that Miss Calkins' conception of consciousness and her conceptions of the characters of the self move in a circle. We start with consciousness as the relatedness of the self, immediately experience the self as possessed of four characters which, as we have seen, all reduce to one, namely, to relatedness and so are back at the starting point without appreciable gain.

### *The Self as Knower and as Known*

There is a further point which Miss Calkins does not make clear; namely, the relation between the self as knower and the self as known. We have every right to ask for this distinction, because throughout the system it is implied. For example: "A person or an impersonal fact, to which the self is related, is called its object—that of which it is conscious . . . I am always . . . conscious of the private personal object, myself . . . There are certain externalized objects, in particular my body, of which I am so persistently and attentively conscious that I often seem to regard them as part of myself. And, finally, preëminent among the objects of my environment, are the other selves."<sup>57</sup> "The standpoint from which one speaks of objects of the self is, as James says, dualistic. The basis of the conception is the fact that I always find myself conscious of an object: of myself or my experience, of other self or thing or relation . . . Psychology, if it is to take account of the self, must, therefore, take account of the object."<sup>58</sup> If psychology is, then, to study both selves and objects of selves, which may be the selves themselves, we might expect that there should be some differentiation of the two, so that we might know whether we were talking of selves, or of selves as objects of themselves at the moment. But, on the contrary, nowhere in Miss Calkins' psychology are clear distinctions drawn between the self as knower and the self as known. When considering the question of subject and object she writes: "I am always, inattentively or attentively,

<sup>57</sup> *First Book*, 1914, 3 ff.

<sup>58</sup> *Op. cit.*, 280.

conscious of the private, personal object, myself, whatever the other objects of my consciousness . . . It must be pointed out that certain real difficulties attend the classification of the self's objects. There is . . . the difficulty of conceiving the self as both subject and object . . . But these are difficulties only for the metaphysician. The psychologist, who like every scientist, must accept certain facts, without looking for their ultimate explanation, rests . . . on the immediate certainty that I am conscious of myself."<sup>59</sup> After learning that psychology studies both a knower and a known, we are thus told that we should ask no more about them.

If we turn, now, to Miss Calkins' philosophy for the information which she withholds in her psychology, we find that: "In being conscious of a phenomenal fact, the subject (or knowing self) certainly does know an object different from a self. This does not argue against the existence of another sort of knowledge, in which there is no recognition of subject or object—in which, rather, subject and object coalesce in the experience of my consciousness of myself, as knowing and thinking, feeling and willing."<sup>60</sup> However, on returning to the psychology, we discover that: "I always find myself conscious of an object: of myself or my experience . . ."<sup>61</sup> In Miss Calkins' philosophy, then, there is no subject-object relation in the "experience of my consciousness of myself as knowing, etc.;" while, in her psychology, there apparently is such a relation.

If we continue reading the "First Book" we become more and more confused; for there the "psychological self" is identified with the subject, or the self as knower.<sup>62</sup> It is, of course, not necessary for psychology to take up the problem of knower and known; but it is evident that, if a psychology continually suggests such a distinction, we should expect its author to state which aspect she is considering. It may also be suggested that the question whether the self is knower or known, or both in coalescence, is just as "scientifically relevant" as the question "Whose idea is this idea?"

### *Difficulties in the Concept of the Self*

It is evident from this discussion of Miss Calkins' self that the concept is an extremely complex one, and one from which it is difficult to pick out the important points. We have seen

<sup>59</sup> *Op. cit.*, 3 ff.

<sup>60</sup> *The Persistent Problems of Philosophy*, 1910, 351 f.

<sup>61</sup> *First Book*, 280.

<sup>62</sup> *Op. cit.*, 281.

that the general concept of the self as a fact, as immediately experienced, as the "plain man's self," is far from clear; that persistence, uniqueness, and complexity can not be distinguishing characters of this self; that, under Miss Calkins' definition of consciousness, relatedness must really include the other three characters and, for that matter, the self itself;<sup>63</sup> and that a consideration of the characters of the self has, after all, done little to clear the concept of the self.

We find, however, still other difficulties in the concept. First, various arbitrary limits seem to be drawn in the study of the relations of the self. For example, we find from the Index of Subjects in the "First Book" that we study the self as "persistent . . . complex . . . unique . . . related . . . as egoistic . . . as altruistic . . . as particularizing and individualizing . . . as generalizing . . . as receptive or passive . . . as active or assertive . . . as perceiving and imagining . . . as recognizing . . . as thinking . . . as emotional . . . as willing . . . as believing . . . as social . . . as religious . . . as object . . . as subject . . . secondary or subconscious self." There seems to be no reason for stopping here. Why should we not go on and discuss the self as singing, as teaching, as philanthropic, as atheistic, as measuring the declination of a star, and so on? For "From the conception of psychology as science of myself in relation to my environment, personal and impersonal, it follows that every concrete personal relation may be the basis of a psychological study."<sup>64</sup> Even with the subject-matter of psychology defined as relations of the self, we are unable to decide what topics it should consider; for Miss Calkins' table confuses rather than clears up the question. The main heading of the table is: "Myself (as subject) is related to (or, conscious of) objects:"<sup>65</sup> This, then, is evidently the subject-matter of self-psychology. But no! for these objects, to which the self is related, are further differentiated into objects of Social Sciences, of Logic, Mathematics, etc., of Physical Science, and of Psychology. Psychology seems here to be made a subdivision of itself, just as, it will be remembered, relatedness was made a subdivision of relatedness.

A more fundamental and a more insistent difficulty is that

<sup>63</sup> If consciousness ("First Book," 1) always "a somebody-being-conscious," then it cannot be true that (*Psych. Rev.*, 13, 1906, 68) "to be conscious is to be conscious of a conscious self." This statement should, on the contrary, read: "to be conscious is to be a conscious self."

<sup>64</sup> *First Book*, 260.

<sup>65</sup> *Op. cit.*, 1914, 4.

the concept of the self is a metaphysical and a logical, not a psychological concept. That it must be a metaphysical concept, if the "plain man's self" is taken as the self, we have already tried to show. That it is a logical concept becomes evident when we find that Miss Calkins is not merely scientifically describing the self, but is rather introducing *meaning* into her concept, is *interpreting* the self. It is the *meaning* of the self that is of prime importance to the plain man; and it is, in the same way, the meaning of the self that is of prime importance to the self-psychologist of Miss Calkins' type.

This criticism has already been made by Pillsbury. "The self, as developed socially, is an interpretation . . . The data that are interpreted we find first, probably, in the constant mass of sensations, strains . . . About these group the socially recognized differences from other individuals, and out of the mass there precipitates an awareness of the self as a meaning."<sup>66</sup> Titchener's criticism of "Der doppelte Standpunkt" pursues a similar line of thought. "With the admission of relational elements into a psychological system the system ceases to be psychological, and becomes so far logical . . . The relational element, in my opinion, is born of the spirit of the older functional associationism, the besetting sin of which lay in its confusion of fact with meaning."<sup>67</sup> The self itself, as well as the relational elements, make the system logical and not psychological; if you start in with *self-meaning* for analysis, as Miss Calkins does, then you must get *relational meanings*. If a "plain man" were asked casually whether or not he considered himself to be persistent, unique, complex, and related, he would possibly agree that he did. He would possibly also agree that he was always changing, was very similar to all other selves, was a unit, and was, in a way, unrelated. In other words, the plain man would accept any meaning which could be applied to his self at some particular time, and would not question the consistency of the characters of the self which he thus obtained. Such a self is, however, not the self directly experienced in those situations in which consciousness of self is very evidently present; and the difference is not merely a difference of degree.

Miss Calkins herself at times acknowledges that she is interested in the logical self; the primary interest of self-psychology is to "understand, not to analyse into elements," that is, to get the meaning, the logical import, not the psychological description. Again we read: "The contemporary functional

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<sup>66</sup> *Phil. Rev.*, 16, 1907, 402.

<sup>67</sup> *Phil. Rev.*, 15, 1906, 93.



psychologist, not content with describing consciousness as reaction to environment, commonly lays stress on the 'value,' 'meaning,' or 'utility' of the reaction."<sup>68</sup> But, remember, functional psychology pruned of biological excrescences is self-psychology; so, then, self-psychology lays stress on 'value,' 'meaning' and 'utility.'

### *The Method of Self-psychology*

The method peculiar to all psychology is introspection. So far Miss Calkins agrees with structural psychologists. But upon further inquiry it develops that by the term introspection Miss Calkins does not mean 'attention and report under instruction,' as the structural psychologist does, but rather reflection. "To observe myself perceiving, remembering, or judging is no longer simply to perceive, to remember, and to judge, but to reflect upon perception, memory, and judgment."<sup>69</sup> Moreover, she holds that the methods of science in general are to describe exactly and to explain, if possible, the phenomena which are observed.<sup>70</sup> Describing consists of analysing and classifying; therefore, if psychology is a science, we should expect its method to be observation, description, and explanation. We learn, however, that the method used in self-psychology is different from that used in structural psychology; but exactly how it is different, we find it difficult to discover. In "Psychology as Science of Selves"<sup>71</sup> mention is made of the wide divergence in method of the two kinds of psychology; but we learn nothing of the method of self-psychology. Later we are told that every conscious experience may either be "dissected by the structural psychologist and described as a complex of elements"<sup>72</sup> or may be "frankly acknowledged as experience of selves . . . The more closely . . . we observe ourselves perceiving, the more surely we discover in the background of experience the consciousness of other people, actually or conceivably present, who see what we see." How precisely does the method used to find "in the background of experience the consciousness, etc." differ from the method of structural psychology? Again it is asserted that selves are to be "studied in their diverse relations to each

<sup>68</sup> *Journ. of Phil.*, etc., 4, 1907, 681.

<sup>69</sup> *First Book*, 7. While it is true, of course, that reflection precedes and follows the application of scientific method, it is not true that reflection is a method of science. The method of science is observation.

<sup>70</sup> *First Book*, 1.

<sup>71</sup> *Phil. Rev.*, 9, 1900, 494.

<sup>72</sup> *Op. cit.*, 497 ff.

other and to facts of other sorts."<sup>73</sup> And we learn that "a self-being-conscious is not only analysable into elements but is also a complex of relations to its environment."<sup>74</sup> In all this there is nowhere a clear statement of method. In other articles we read that "self is *found to have* [italics not in original] certain characters,"<sup>75</sup> but *how* it is found to have them we can not discover. We read that the merely structural analysis is one of the "several essential analyses,"<sup>76</sup> but we never learn what the other essential analyses are. We read that the description peculiar to self-psychology is a description of consciousness "in terms of the characters of the conscious self."<sup>77</sup> This, to be sure, gives some insight into the *problem* of self-psychology; but, after all, it tells us nothing of the method by which we are to arrive at the description. *How* are we to discover the characters in terms of which consciousness is to be described?

Some suggestion of the method by which we acquire knowledge of the self is given in the sentence: "implicitly, if not explicitly, I am always conscious of a self as having the idea or experience."<sup>78</sup> But what is the state of affairs when we are "implicitly" conscious of the self? According to dictionary definitions, if something is "implicated" or "implied," that something is obtained by a process of reasoning; and if Miss Calkins wishes to employ a term in a sense different from that in ordinary usage, she should define it anew. The specific method of psychology has nothing to do with logical reasoning and logical inference, as Miss Calkins herself affirms when she says: "I immediately experience myself as, etc." So we must leave the "implicit consciousness of self" out of the question; it offers no basis for a psychological system.

We may summarize what we have learned about the method peculiar to self-psychology. (1) The method peculiar to all psychology is introspection; (2) the method of self-psychology is widely divergent from the method of structural psychology; (3) there are other analyses beside structural analyses; (4) every complex experience is both a complex of structural elements and a self related to environment. We must, then, find the method for ourselves. The one difference between the method of self-psychology and that of structural psychology which we find in Miss Calkins' writings is the difference in the

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<sup>73</sup> *Op. cit.*, 501.

<sup>74</sup> *Psch. Rev.*, 13, 1906, 63.

<sup>75</sup> *Journ. of Phil.*, etc., 5, 1908, 65 ff.

<sup>76</sup> *Loc. cit.*

<sup>77</sup> *Journ. of Phil.*, etc., 5, 1908, 113.

<sup>78</sup> *First Book*, 274.

meaning given to the term 'introspection.' Miss Calkins uses the term as a synonym for reflection; the structural psychologist uses it as a synonym for observation. The method of self-psychology, therefore, seems to be reflection. This conclusion is sustained by various statements of our author's, such as "The description of imagination as self-consciousness is . . . in exclusively reflective terms."<sup>79</sup> But reflection is the method of logic, and not of science. The conclusion that the method peculiar to self-psychology is the method of logic is, of course, what we should expect if we accept the more general conclusion that the concept of the self is a logical concept, and that the characters of the self have been obtained by logical reasoning.

### *The Problem of Self-psychology*

The problem of self-psychology is never directly stated by Miss Calkins, although we may assume, first, from her discussion of method, that psychology is to describe accurately, to classify and to explain; and from the "First Book," that it hopes to gain "a fuller understanding of these relations [of self to other selves] in their complexity and a deeper acquaintance with my own nature."<sup>80</sup> Moreover, we have self-psychology referred to as "the science of consciousness whose 'primary interest is . . . to understand . . . not to analyse into elements.'"<sup>81</sup> From such statements we must conclude that the problem of self-psychology differs from that of structural psychology in that, besides attempting to describe, it aims also to understand, to obtain a fuller understanding of the relations of selves, and to acquire a deeper acquaintance with one's own nature. But with this problem of understanding and interpreting, self-psychology can scarcely claim a right to the name of science. The problem of a science is to describe accurately the phenomena which it observes;<sup>82</sup> self-psychology has an additional problem, to understand and interpret the relations which it knows immediately or *reflectively*. The opposition of the two is clear.

It is interesting in this connection to continue the quotation

<sup>79</sup> *Journ. of Phil., etc., op. cit.*, 117.

<sup>80</sup> P. 10.

<sup>81</sup> *Phil. Rev.*, 9, 1900, 494. The phrase 'primary interest is . . . to understand . . . not to analyse into elements' is quoted from Münsterberg's "Psychology and Life." Münsterberg is speaking of history; but in the preface to Miss Calkins' "Introduction to Psychology" we read: "The conception of psychology as a science of related selves is closely affiliated with Münsterberg's conception of history as science of the relations of willing subjects."

<sup>82</sup> *First Book*, 1.

from Münsterberg which Miss Calkins begins. After speaking of the science whose "primary interest is . . . to understand . . . not to analyse into elements," Münsterberg goes on: "We want to interpret their meanings and not calculate their future . . . All we want to know about them is with what other attitudes they agree or disagree. We have the logical aim, to consider them in their relations to all other will-attitudes and to work out the system of these connections." The problem of self-psychology is, then, for the author of the expression "to understand not to analyse into elements," a logical problem. And not only is the problem of understanding and interpreting relations a problem of logic rather than of psychology; but the further problem (not definitely stated, but hinted as an outcome to be expected) of applying the knowledge thus acquired is also a problem for a technology rather than for a science. In the passage quoted above: "A fuller understanding of these relations . . . and a deeper acquaintance with my own nature may surely, therefore, have a significant influence on my behavior."

It is evident that the problem of self-psychology is much broader than the problems of most psychologies. It is one, moreover, that not only transcends the limits of psychology, but that also infringes on the territory of other disciplines, particularly those of logic, epistemology, and the technologies.

### *The Results of Self-psychology*

The results attained by self-psychology are, in their general nature, what we should expect from the subject-matter, method, and problem. Aside from incidental "structural analysis," the "First Book" gives us detailed and careful statements as to the meaning, significance, and use of various experiences. As the method of self-psychology is to describe and interpret in terms of the characters of the self, the results of self-psychology will, of course, be expressed in these terms

Perception is described as "sensational, passive consciousness reflectively realized as impersonal, particularizing, common to other selves, and as related to present external objects." Imagination is described as "sensational, impersonal, and particularizing, but as lacking the consciousness of passivity, the relation to present objects, and the community." The only characteristics of perception, then, which are "immediately realized" are that it is sensational and passive. The passivity of the self is defined as a 'personal attitude' or 'relation of the self to objects.' The statement that the passivity is immediately experienced is contradicted by a statement made elsewhere,<sup>83</sup> in which Miss Calkins grants that there is no "immediately experienced difference between perception and imagination."

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<sup>83</sup> *Journ. of Phil., etc., loc. cit.*

Attention is stated to be "a unique attitude, a basal relation of self to object, comparable with receptivity or sympathy or activity. This conception may, but need not, be combined with the teaching that there is a structural element of clearness, or attended-to-ness."<sup>84</sup> Here again we have, as we expected, relations of the self. The expression "basal relation" raises the question of the difference between basal relations and other kinds of relations, and how the two are discovered and differentiated; but these questions are not answered.

Recognition is also discussed as "personal attitude;" whether basal or not, is not said. Under the section entitled "Recognition as Personal Attitude" the essential characters of recognition are formulated; the most important is the "emphasized persistence of the self" (another argument for the theory that relatedness really includes the other characters). Upon structural analysis, recognition is distinguished by the prominence of relational elements. The recognizing self is said to be "(1) relationally conscious of, (2) itself as persistent and of objects as related to its past." It is not clear why this sentence should not have read: "the recognizing self is relationally conscious of itself (1) as persistent and (2) as related to objects in the past, or related to its former self as related to objects." Under such an arrangement, the whole description of consciousness would be in terms of the characters of the self, and the necessity of bringing in "structural" elements would be avoided.

In the chapter on thought: "The thinking self is the self (1) relationally conscious (2) of related objects which (3) it knows, reflectively if not immediately, as objects, also, of other selves." And again, there are relational elements in thought. "Conception is the relational consciousness (reflectively attributed to other selves also) of a group or of an object as member of a group." "By 'judgment' . . . is meant the relational consciousness of a whole as including or excluding certain emphasized features, an experience reflectively known as sharable with other selves."

Emotion is discussed as personal attitude and as affective consciousness. Under personal attitude we find that emotion is, "first and foremost, an intensely individualizing experience. . . . In my emotion . . . I immediately realize myself as a unique self . . . and . . . I individualize the object of my love or hate or fear. . . . A second character is its receptiveness." As affective process, emotion is characterized by relational elements, affective elements, and organic sensations.

"Will" is described as "an egotistically assertive consciousness. It is obviously a profoundly attentive and a doubly individualizing experience. . . . From objects of thought of which one is aware as related primarily to each other, objects of will, like objects of emotion, are sharply distinguished in that they are realized as related to the self . . . and as in a way dependent on the willing. . . . The willing consciousness includes . . . sensations of movement . . . the consciousness of (1) futurity, (2) of connectedness, and (3) of realness—experiences corresponding with the characters . . . of the willed object."

Faith and belief are described as "assertive, doubly individualizing adoptive attitudes to objects of any sort, and as distinguished by the elemental consciousness of realness and by that of congruence." Under the heading "religious consciousness" religion is defined as "the con-

scious relation of human self to divine being, that is, to a being or beings regarded as greater than this human self, or than any of its fellows, and either conceived or treated as personal."

In addition to those sections of the book which deal with descriptions of the various relations of the self in the manner illustrated above, we find other passages entitled: "The Uses . . . of Memorizing," "The Uses and Dangers of Conception," "The Uses and Dangers of Reasoning," "The Significance of Emotion." Here we have, evidently, the fulfillment of the aim to obtain "a deeper acquaintance with my own nature" which "may surely . . . have a significant influence on my behavior."

These results of self-psychology, aside from their general nature and the terms of their expression, are surprising for two reasons. First, since structural psychology is at so great pains to standardize conditions, to repeat experiments many times, and to obtain many observers for every detail of its results, it is amazing to find the results of self-psychology given with such definiteness, with such certainty, when apparently Miss Calkins herself has been the only observer, and we are told nothing of the conditions under which she worked, and are unable to verify her results. In the second place, it is strange that, in a psychology so different from structural psychology as is self-psychology, only the topics familiar from structural psychology are discussed. It will be remembered that, in the Index of Subjects to the "First Book" the self was referred to in many relations; though we might have expected it to be studied in many more relations. Instead, now, of finding the self discussed "as active," and so on, the main topics considered are those which occur in any psychology: perception, imagination, memory and so forth,—instead of the self as perceiving, as singing, and all the rest. However, selection of topics is probably due to the fact that tradition has more or less prescribed the topics for psychology in general.

We pass on to consider the significance of the results obtained. In addition to "structural analysis" we find, first, a description of the varying relations of the self. This description is obviously not the kind that may be obtained immediately from observation. It is, on the contrary, the reflective description which an "arm-chair" psychologist could make as well as, if not better than, a psychologist who attentively observes his actual experiences. Any logician, or any "plain man" who is skillful in drawing distinctions of meaning, could distinguish, for example, between "happiness in another's happiness" and "happiness in another's unhappiness;" be-

tween an "assertive attitude with self as object" and "an assertive attitude with other self as object;" between "perception regarded as shareable" and "imagination regarded as unshareable." But if such distinctions can be drawn by one versed in logic and ignorant of psychology, it is at least possible that logic has been introduced into self-psychology to obtain its results.

The second result of self-psychology is the application of results of the first kind. Application, however, belongs not to science, but to a technology. Physics leaves the application of its facts to engineering and the allied technologies; similarly, psychology should leave its applications to its allied technologies, such (Miss Calkins has herself suggested) as ethics, social science, and pedagogy.

### *Conclusion*

Let us now see whether the claim that psychology is "most naturally, consistently, and effectively treated as a study of conscious selves in relation to other selves and to external objects" has been justified.

In the first place, we must repeat that an evaluation of the system is difficult, by reason of the lack of any method by which we might reach Miss Calkins' results. If she would tell us *how* to get the self, and *how* to get the characters and the relations, how in a word her conditions are to be duplicated, we might then have results of our own which should be comparable with hers. In the absence of any such method, we have found that only by a process of logical inference do we arrive at her self with its characters.

Another difficulty is that, no matter how vigorously many psychologists deny the ever-present consciousness of self (which is the apparent excuse that self-psychology has for being),<sup>85</sup> Miss Calkins insists that these psychologists really are self-psychologists, because they at least imply the self "as basal fact." She writes:<sup>86</sup> "The idea-psychologists are implicitly assuming or leading their readers to assume the existence of a self, when they describe consciousness in such words as 'I attend to a color' . . . and still more when they mark off certain experiences as peculiarly personal, that is, as espec-

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<sup>85</sup> Personally, I cannot see why—if we *were* conscious of self all the time—that should necessitate a superior psychology of selves; any more than the fact that we were "inattentively or attentively" conscious of our body all the time should necessitate a superior psychology of bodies.

<sup>86</sup> *First Book*, 279.

ially related to myself."<sup>87</sup> The ground for the assertion that idea-psychologists "unambiguously imply the consciousness of self as part of the experience described in terms of the idea" is apparently the fact that they, like all the rest of the world, use the personal pronoun. The argument, on its most favorable interpretation, is a broken reed. For it is one thing to say that "idea-psychologists . . . assume the existence of a self," and another thing to say that they "imply the consciousness of self as part of the experience . . . ." One may "assume the existence of a self" without assuming that one is always conscious of that self; one may assume the existence of the mathematical proof of a formula without being aware of that proof. As to the personal pronoun,—if the use of it makes one a self-psychologist, in Miss Calkins' sense, then her own use of such words as 'garden,' 'motor-car,' 'forehead,' makes her a materialist.

We conclude that Miss Calkins' arguments from "the self as basal fact" are not convincing. We have already suggested (p. 80) that the self of self-psychology seems to be another name for the all-inclusive mental function of functional psychology; and structural psychology can hardly be expected to treat of that self. But, if we cannot accept Miss Calkins' statement that all psychologists are really self-psychologists, can we accept the other statement that "psychology is most naturally, consistently, and effectively treated" as self-psychology? We consider these points in order.

How is psychology most "naturally" treated? How, for that matter, is anything most "naturally" treated? The answer is, apparently that a thing is treated most naturally when treated as the plain man would treat it. There can be no doubt that the plain man would more often consider the relations of persons (or selves, if they must be deprived of body) to each other and to impersonal objects, than he would consider mental processes as such. But then psychology is a science, and not mere common sense. And this puts the matter in a very different light: for no science is "natural" in the sense given above; if it were, it would not differ from casual observation and interpretation; science is, on the contrary, necessarily abstract and, as Miss Calkins would say, artificial.

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<sup>87</sup> Miss Calkins is here referring to the statement made by Titchener: "regarded from the point of view of ordinary life, blue and warm are somehow detachable from oneself, whereas pleasantness is always within oneself." But Titchener does not make the statement "from the point of view of psychology;" and the "plain man's self" surely may not be carried to the point of accepting in psychology distinctions which it makes in 'common sense.'



Science does not aim or hope to be either "natural" or non-abstract. If, then, self-psychology is more "natural" than other psychology, in the sense of standing nearer to the view of the plain man, self-psychology loses thereby rather than gains; for the more "natural" or common-sense-like it gets, the less scientific it becomes.

As for consistency: we have shown that the characters of the self are not consistent, and do not fulfill the duty demanded of them (that of differentiating between self, and idea and function); we have shown that the system is not carried to logical completion. Moreover, as we have already suggested, the topics of self-psychology are surprisingly like those of structural psychology. With fundamental conceptions as different as they are, we should naturally expect the two psychologies to deal with different topics. Self-psychology gives us activity, passivity, egoism, and a number of 'personal attitudes' of which we hear little or nothing in structural psychology; and yet we find no new topics, no new chapters. It is a real question whether Miss Calkins has consistently worked out the results which should follow upon her premises; and the answer to the question is, again, bound up with the exhibition of her method.

The third advantage claimed for self-psychology is effectiveness. The meaning of this term, again, is not very clear. But if it means the production of demonstrable results, we may look for such results either within or without the given science. A science, to be effective within its own limits, must provide stimulus and suggestion for further work. Self-psychology, now, sets out to describe and interpret certain relations of the self in terms of the four characters of the self. Let it be granted that the paragraphs of the "First Book" are both complete and consistent; and let it be granted that they could not have been written by a logician, by any one who was not a psychologist: still, only a few of the relations of the self have been discussed, and there is further work to be done if all the relations are to be handled. That is, so far, promising. But Miss Calkins tells us that it is impossible even to enumerate all these relations. So the further work, to which alone, by hypothesis, we are stimulated, is at the outset acknowledged to be hopeless; and we therefore have small enthusiasm to pursue the method of self-psychology further. As for the outside influence of self-psychology, that is to be exerted in "ethics, social science, and pedagogy." But as these disciplines are, at least in a certain stage of their development, older than self-psychology, it remains a question whether they may not

have helped to shape self-psychology as positively as self-psychology has aided or promises to aid them. Moreover, this sort of argument has, as we have seen, another edge; for application places self-psychology among the technologies, and behind technology there must always be science. So far, then, it seems that the claim of effectiveness remains to be clarified and substantiated.

We must conclude, therefore, that, of the three ways in which Miss Calkins claims self-psychology as superior to other forms of psychology, naturalness is an inferiority in a science rather than a superiority; self-psychology is not effective in the sense that it encourages further work; and finally self-psychology has not been made thoroughly consistent. In the second place, if we may judge it by ordinary standards, self-psychology has a subject-matter which is largely logical and metaphysical; employs a method which is reflection, the method of logic; formulates a problem which is a problem of logic and of technology as well as a psychological problem; and furnishes results which might be obtained without psychological training, simply by a process of reasoning.

# AN EXPERIMENTAL STUDY OF SENSORY SUGGESTION

By A. S. EDWARDS<sup>1</sup>

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## A. INTRODUCTION

Many definitions have been offered of the term Suggestion, and many experiments have been made in whose results the effects of suggestion have found qualitative or quantitative expression. But no one, we believe, has attempted by means of careful introspection to study the suggested consciousness itself. It is matter of common experience that suggestion may arouse, deflect or inhibit movement; it is equally clear that suggestion may change the meaning of a perception or idea by shifting its context, that is, by giving it a new associative setting. But if we ask whether or to what extent suggestion may bring about intrinsic changes in conscious contents, we receive only partial and incidental answer. A continuously rising tone, for example, is presented under the suggestion that the pitch is falling. The observer reports a drop in pitch. Has he, then, heard what he reports, or is he subject to an illusion of judgment?

In connection with sensation, especially, do we find equivocal statements and a lack of experimental study. We find statements that seem to indicate a direct sensory result of sug-

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<sup>1</sup>From the Psychological Laboratory of Cornell University.

gestion. Baldwin, in the *Dictionary of Philosophy and Psychology*, mentions a 'sensory' suggestion; and the illustration given is that of a person who 'sees' a red light as green.<sup>2</sup> But the question remains whether, in such an instance, the observer actually sees green when red is presented; or whether he 'imagines' that he sees green; or whether, after all, the effect is motor, and he makes his reply not as the result of a sensory experience, but simply as a motor discharge which was predetermined by the suggestion. In experiments with suggestion, Scott finds 'illusions' of heat and light;<sup>3</sup> Gilbert reports 'illusions' of weight and sight;<sup>4</sup> Katz writes that a presented blue appeared dark-brown;<sup>5</sup> Seashore maintains that suggested illusions are surprisingly realistic.<sup>6</sup> But in no case do we find a thoroughgoing analysis and methodical control of the processes actually present in the minds of the observers. Külpe writes: "The inference is, perhaps, allowable that in certain circumstances they [centrally aroused sensations] are qualitatively similar to peripherally excited contents." But he concludes that the existing evidence from experiment is unsatisfactory; the experiments that he refers to in this connection, he thinks, "cannot be considered as wholly free from objection."<sup>7</sup>

It is true that hypnotism has been appealed to, in proof of the possibility of producing sensations by suggestion. The hypnotized subject appears to suffer pain, to experience feelings, pleasant or unpleasant, to realize any sensation suggested to him; indeed, Külpe affirms that to suggestion "every organic function, motor and sensory alike, is subject . . . The wildest illusions of sense and the most absurd actions are evoked with equal ease."<sup>8</sup> But sensory results from hypnotized subjects, even if they were fully proved,—as they seem not to be,—could point only to the likelihood, not to the certainty, of similar results in the normal states of consciousness.

Our appeal, then, is to the laboratory and to introspection; and certain factors in the situation must especially be taken

<sup>2</sup> J. M. Baldwin and G. F. Stout, *Dictionary of Philosophy and Psychology*, ii, 1902, 619.

<sup>3</sup> W. D. Scott, Personal Differences in Suggestibility, *Psychol. Bull.*, 7, 1910, 5 ff.

<sup>4</sup> J. A. Gilbert, Mental and Physical Development of School Children, *Yale Stud.*, 2, 1894, 44.

<sup>5</sup> O. Katz, Versuche über den Einfluss der "Gedächtnisfarben" auf die Wahrnehmungen des Gesichtssinnes, *Zentralb. f. Physiol.*, 20 (16), 1908, 517.

<sup>6</sup> C. E. Seashore, Measurement of Illusions and Hallucinations in Normal Life, *Yale Stud.*, 3, 1895, 62 ff.

<sup>7</sup> O. Külpe, *Outlines of Psychology*, 1909, 183.

<sup>8</sup> *Ibid.*, 453.

into account. Since there is a natural disposition to give positive answers, it is necessary to prove that the results do not express the mere ripening of motor tendencies. It seems not at all unlikely that a suggestion may set up a definite tendency to report in a certain way, and that the appropriate situation (the critical term in a series of stimuli) is then adequate to touch off the motor response. Again, it is possible that the supposed sensation is in reality an image; the modifying effect of expectant attention must be taken into account. Abnormal conditions, such as exhaustion, morbid emotivity, etc., must, of course, be eliminated.

From this brief introduction we proceed at once to a statement of our special problem, methods and results. A history of suggestion would be blind, and a critical sifting of the outcome of previous experiments on suggestion is impossible, until we gain a positive insight into the nature of the suggested consciousness; these, then, are topics that we reserve for a future paper. Our present results are themselves hardly more than preliminary; the experimental work requires much time, and is often inconclusive; but we hope that other investigators may be attracted to the field, and that data may presently be available that shall justify a physiological theory.

#### B. PROBLEM OF THIS STUDY; METHOD, APPARATUS, OBSERVERS

Our general problem is that of the nature and extent of the intrinsic changes produced in consciousness by the influence of verbal suggestion. We are especially interested in sensory results, such results as are normally aroused by an adequate sensory stimulus. The experiments are, therefore, planned to favor sensory results. The questions for which we seek an answer are the following: Can verbal suggestion do the work of an adequate sensory stimulus? What percentage of judgments can be inverted, when supraliminal charges of sensory stimuli are used? How far above the limen can we get these inversions of judgment? What are the conscious processes present when such inversions are made?<sup>9</sup>

The experiments fall into three groups: (1) the suggestion group, (2) the control group, and (3) the liminal group.

In the first group, the observers were called upon to discriminate intensities, qualities, etc., under the guidance of a misleading suggestion; for example, a tone rose in pitch when they were told that it would fall. The first trial in every series was made with a minimal difference of stimuli, and the difference was increased in succeeding trials. *E* continued to increase the stimulus-difference until he feared

<sup>9</sup> In regard to the affective processes, we merely raise the question whether the effect of suggestion is not secondary, following upon the arousal of organic sensations. We have no experimental data to offer.

that the observers would detect the deception. Suspicions on the part of the observers were unavoidable; they had to be assured again and again that *E* was giving the stimuli in accordance with his original statement. Whenever there appeared results of suggestion strikingly like the results ordinarily obtained from adequate sensory stimuli, introspections were taken, and the experience was compared with the normal: details will be given later. In the second group, the observers made the same discriminations under the regular methodical instructions. They now made them, however, with full knowledge of the suggestions employed in the first group, and with full assurance that no misleading suggestion would henceforth be used. Further, they were required to give full introspections of the fore and midperiods, with constant reference to the conscious effect of suggestion in the parallel experiments of the earlier group. The third group consisted of determinations of difference limens, by the method of limits, taken in order that the results of suggestion in the first or suggestion group might be subdivided, the trials in which the stimulus-differences were subliminal being set apart from those which gave inversions of judgment when the stimulus-differences were supraliminal. It is this latter set, in which we have what may be called supraliminal inversions of judgment, that interests us in the present paper. *E* took as the difference-limen the first difference of stimuli, correctly judged, which had two correct judgments beyond it. This is a slightly larger limen than is obtained by other forms of procedure; but the larger limen was deemed desirable, since comparison with the results of the suggestion group demanded a margin of safety. In order to make the results of the suggestion and of the liminal groups comparable, the steps in the corresponding series were kept the same. The liminal series served also to assure *E* of the normal acuity of the observers for the sense departments investigated.

Experiments 7, 8, and 9 were complete without the control group. In the remaining experiments, the control followed the suggestion group in inverse order of series. If this arrangement made the burden of memory greater for some, it greatly relieved it for other experiments; and we considered it as, on the whole, the best plan to adopt. The control for experiment 3 came immediately after the parallel suggestion experiment; for 11, there was an interval of a week; for 2 and 3, four and five weeks (3 months for one observer); for 10, two months; for 1, three months; and for 6, one, four and five months for the various observers.

Experiments were made—not all, as we shall see, with the same thoroughness or success—upon sight (quality, intensity, extent), hearing (noise intensity, tonal quality and intensity), taste (quality), smell (quality, intensity and duration), pressure (intensity), and temperature (intensity of warmth and cold). There were four experiments upon sight; three upon hearing; one upon smell; one upon taste and smell; and one each upon pressure and temperature. Most of the experiments were planned for inversions of judgment; in two cases, however,—one of the audition experiments and the smell experiment,—the attempt was made to get, by means of verbal suggestion, the process ordinarily aroused by adequate stimulus, and to get it in the absence of any adequate sensory stimulation whatever. In three cases, owing to the nature of the experiment, to lack of time, or to difficulty with apparatus, we failed to obtain liminal determinations.

For vision, colored, black and white discs were mounted on the Marbe mixer;<sup>10</sup> recourse was also had to Whipple's apparatus for the discrimination of brightness.<sup>11</sup> For visual extent, we used Wundt's space-limen apparatus.<sup>12</sup> The Stern variator, Fechner pendulum and modified Lechmann acoumeter<sup>13</sup> were employed for sound experiments. Two paper funnels were used in the smell experiments; small candies for taste and smell; the Wundt pressure apparatus<sup>14</sup> for pressure, and brass cylinders for temperature. Continuous changes were possible with the Marbe mixer, the Whipple box, and the Stern variator.

In experiments of the first and second groups (with exceptions noted later), the observers were told, either that a stimulus would be given and that shortly afterwards a second would follow, or that a continuously changing stimulus would be given; they were to judge the direction of the change and, when asked, to give full introspections; the suggestion as to direction of change was made just before each trial. The suggestion always ran counter to the facts; so that if *E* said the change would be toward brighter, he changed the stimulus toward the darker. The observers were informed that the first changes would be small, and that the series would be continued until they could clearly perceive the change suggested. In the experiment on smell, the observers were told that they would be given an odor and that, as soon as they indicated their perception of it, a second odor would be given which should neutralize the first. No odors were used, and no experiment was begun until the observer reported the room free from all odors. In the experiment on taste and smell, the candies were given in pairs, the one to be eaten shortly after the other; the two differed in shape, and one was so wrapped as to appear of better grade than the other. The ingredients were the same in both cases. The suggestion lay in the appearance of the candies and in the verbal instructions; the observers were to write full introspective comparisons of the sensations of taste and smell aroused by the two kinds. In one of the audition experiments, a lead and a cotton ball were used, the sight of the falling cotton ball being the suggestion for the sound. The object of these experiments was, then, either to arouse through suggestion the process commonly aroused by an adequate stimulus, or by means of suggestion to invert judgment. The omission of several steps at the end of a series permitted the experimenter to obtain, in a few instances, very large supraliminal results, and at the same time to avoid the danger of detection, which was always present, but was especially to be feared in the longer series.

The experiments were carried on with five men in the Cornell laboratory during the greater part of two years, and later

<sup>10</sup> K. Marbe, *Physiol. Centralb.*, 1894, No. 25, 811.

<sup>11</sup> G. M. Whipple, *A Manual of Mental and Physical Tests*, 1910, 163.

<sup>12</sup> W. Wundt, *Physiol. Psychologie*, iii, 1911, 466.

<sup>13</sup> E. B. Titchener, *Exp. Psych.*, II., I, 1905, 24.

<sup>14</sup> W. Wundt, *op. cit.*, I, 1908, 674.

with five women at the University of Minnesota during one year. The men will be referred to as *A*, *B*, *C*, *D* and *E*; the women as *F*, *G*, *H*, *I* and *J*. Observers *A*, *B* and *C* had all had thorough training in introspection, *A* and *B* in the Cornell laboratory and *C* in the Harvard laboratory. One was an instructor in the department, one an assistant, and the third an advanced student engaged in research. *D* and *E* were respectively a fellow and a scholar in psychology. *F*, *G*, *H*, *I* and *J* were advanced students in psychology, one being an assistant in the department with some training in introspection; all needed further training.

A number of trials were made with other than the regular observers. These did not call for introspective reports; they were undertaken merely for comparative purposes. They showed that results similar to those obtained from the regular observers could with little difficulty be obtained from persons unconnected with the psychological laboratory and unversed in psychology. Indeed, in some instances, even more extreme results of suggestion appeared with these untrained observers.

In reporting our experiments, we shall first give the quantitative results which show the effects of suggestion in relation to the difference-limen. We shall then turn to an analytical and qualitative study of the conscious processes under the influence of suggestion.

### C. QUANTITATIVE RESULTS

In calling these results 'quantitative,' we do not mean that we have measured the degree of suggestion, or that we have discovered the greatest amount of supraliminal change of stimulus that can be introduced while the judgment of the observer under the influence of suggestion is still inverted. We are only reporting, in numerical terms, the results of particular series of experiments made under particular conditions. These conditions were, in fact, distinctly unfavorable to suggestion. The instructions demanded a critical attitude; the observers were asked to make judgments in series which, so far as they knew, were part of a study of liminal differences, and which demanded their most careful attention. Seven of the ten observers reported suspicions as to the nature of the experiment, and all showed, at times, by action and tone of voice, that they thought the investigation was not what it purported to be. Finally, all such factors as fatigue, excitement, and the like, which are favorable to suggestion, were absent.

The difficulties of the experiments, the suspicions of the observers, and lack of time, made it possible for *E* to use supraliminal charges of stimuli only in 900 of the 1621 trials comprised in the first or suggestion group. In 253 of these



900 trials, the suggestion succeeded in inverting judgment. In other words, suggestion was successful in 28 per cent. of all cases in which supraliminal changes of stimuli were used. This number does not include the judgments 'doubtful' and 'equal.' Of the 254 series in the group suggestion was successful in 110, or in 43 per cent. The following Tables 1, 2 and 3, show (1) the percentage of successes of suggestion with supraliminal changes of stimuli for the various series in the suggestion group, and (2) for the individual experiments and observers, and (3) the number of steps above the difference-limen where suggestion was successful, with the number of the successful cases for every step. To repeat and emphasize, all the results noted in these Tables are results obtained with supraliminal differences of stimuli.

TABLE 1

Number and percentage of series from the suggestion group containing inversions of judgment.

Part 1 According to Experiments				Part 2 According to Observers		
Expt.	Suggestion	No.	%	Observer	Series	%
1	Blue.....	17	71	A	10	25
3	Brighter.....	21	72	B	15	34
	Darker.....	20	74	C	35	42
4	Larger.....	0	0	D	3	100
	Smaller.....	2	20	E	4	100
5	Louder.....	4	80	F	14	86
	Fainter.....	4	80	G	6	35
6	Higher.....	9	45	H	3	50
	Lower.....	..	..	I	10	52
7	Falling shot...	14	16	J	10	48
10	Heavier.....	7	100			
	Lighter.....	0	0			
11	Warmer.....	3	36			

Total series containing inversions = 110 or 43%.

In Part 1, the first column indicates the number of the experiment; the words Blue, Brighter, etc., the suggestion used. The numbers following show first the number (no.) and secondly the percentage (%) of series in which inversions of judgment followed the use of suggestion. E. G., "17 71" means that in Experiment 1 inverted judgments were found in 17 series, or in 71% of all series in the suggestion group.

Part 2 indicates the number and percentage of series containing inversions of judgment for each observer.

TABLE II  
NUMBER AND PERCENTAGE OF SUPRALIMINAL INVERSIONS

Exp.	Suggn.	A	B	C	F	G	H	I	J	Total errors	Av. %	
1	Blue.....	(3 15)	(6 75)	(37 86)	27 44	1 3	11 17	16 76	11 22	66	28	
<hr/>												
3	Brighter ...	4 44	3 15	6 25	18 75	3 15						
	Darker.....	0 0	7 33	8 20	5 50	3 16						
<hr/>												
4	Larger.....	0 0	0 0	0 0						0	0	
	Smaller....	4 36	0 0	4 23						8	21	
<hr/>												
5	Louder.....	1 25	3 14	9 69						13	51	
	Fainter ...	1 17	2 18	10 77						13	43	
<hr/>												
6	Higher.....	1 50	0 0	5 78						6	35	
	Lower .....	0 0	1 25	8 25						9	38	
<hr/>												
7	Shot.....	2 6	3 13	7 13	2 20	0 0	H 0			14	10	
<hr/>												
10	Heavier....		D 40	E 100						8	57	
<hr/>												
11	Warmer....	2 66	1 10	4 66								
	Colder.....	3 75	3 16	6 100						7	35	
<hr/>												
Inversions per observer.....			A 18	B 23	C 67	D 4	E 4	F 52	G 7	H 11	I 41	J 26
Percentage " " .....			20	16	27	40	100	49	8	15	60	27

Total errors for all observers = 253 or 28%

Numbers in the first column refer to experiments 1, 3, 4, etc. The words Blue, Brighter, etc., refer to the suggestions used. The figures in the body of the table show the number and percentage of supraliminal inversions. For example, under A are found the numbers 3 15; these mean 3 errors, which were 15% of the possible errors. The totals and average percentages do not include results for A, B and C in Exp. I, since limens were not

TABLE III

## NUMBER OF INVERSIONS FOR EACH SUPRALIMINAL STEP

## Experiment 1. Suggestion: "Towards Blue"

Degrees:	6	8	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	40	42
A DL.....			1	1	1	1	1	1	1	1									
B DL.....		1	1	1	1	1	1	1	1	1									
C DL.....	2	1	1	3	3	2	1	2	2	1	2	2	1	1	1	1			
F DL 5.1, m.v. 1.5.....		2	4	2	2	2	2	1	1	1									
G DL 7.9, m.v. 1.2.....						1													1
H DL 7.0, m.v. 1.8.....		2	2	2	2	1	1	1	1										
I DL 10.5, m.v. 1.1.....				3	2	1	3	3	2	2									
J DL 7.0, m.v. 1.2.....			1		1	2			2	2	1	1	2	1	1				

## Experiment 3. Suggestion: "Towards Brighter"

Degrees:	4	6	8	10	12	14	16	18	20	22	24
A LDL 9.2, m.v. 3.1.....				1	1						
B LDL 3.7, m.v. 1.5.....		2	1	1	1						
C LDL 2.4, m.v. 1.0.....	1	2	1	1	1						
F LDL 7.6, m.v. 1.2.....			3	4	2		3	2	2		
G LDL 5.8, m.v. 0.4.....				1		2	1			1	
H LDL.....			2	2	2	1	2	1	2	1	1
I LDL 7.7, m.v. 1.1.....			2		2	2	1	3	2		
J LDL 6.1, m.v. 1.9.....			1	3	1	1	1	1	1		

## Experiment 3. Suggestion: "Towards Darker"

Degrees:	4	6	8	10	12	14	16	18	20	22	24
A UDL 3.7, m.v. 1.7.....		1	1								
B UDL 5.3, m.v. 1.0.....		1			1						
C UDL 2.5, m.v. 1.2.....	2	1	3	2							
F UDL 6.8, m.v. 1.8.....			1	1	1	2					
G UDL 7.9, m.v. 1.4.....						1					1
H UDL.....			2	2	2	2	2	2	2	1	
I UDL 8.6, m.v. 1.7.....				1	3	3	2	4	3	1	
J UDL 6.6, m.v. 2.2.....			1	1			2	2			



TABLE III.—*Concluded*

Experiment 10. Suggestion: "Towards Heavier"

Grams: 6 8 10 12 14 16

D LDL 11.0, m.v. 3.4.....  
 E LDL 10.2, m.v. 1.9.....

Experiment 10. Suggestion: "Towards Lighter"

Grams: 6 8 10 12 14 16

D UDL 4.8, m.v. 1.8.....  
 E UDL 8.4, m.v. 1.8.....

Experiment 11. Suggestion: "Towards Warmer"

Degrees: 1 2 3 4 5 6

A LDL 2.1, m.v. 0.7.....  
 B LDL 0.9, m.v. 0.3.....  
 C LDL 0.7, m.v. 0.3.....

Experiment 11. Suggestion: "Towards Colder"

Degrees: 1 2 3 4 5 6

A UDL 2.0, m.v. 0.5.....  
 B UDL 3.2, m.v. 1.8.....  
 C UDL 0.7, m.v. 0.3.....

Table 3 shows the size of the steps, i.e., the amount of supraliminal change of stimulus, for which judgment was inverted under the influence of suggestion. Below the statement of the suggestion are given the various amounts of change of stimulus from the zero point; these are given in degrees, millimeters, etc. The first column indicates the observers; following this are given the difference-limits and their mean variations; the body of the table shows the number of cases of inverted judgment for every supraliminal step used.

## D. QUALITATIVE ANALYSIS OF CONSCIOUSNESS UNDER THE INFLUENCE OF SUGGESTION

### *Experiment 1. Color*

The Marbe Mixer was used for the introduction of yellow by continuous change into red. The change in the first exposure of the series was from  $0^\circ$  to  $2^\circ$ , in the second from  $0^\circ$  to  $4^\circ$ , and so on, the increase in yellow proceeding by steps of  $2^\circ$ . Each series was as long as  $E$  could make it without running an obvious risk of detection. A large gray cardboard served as a screen. The observers seated themselves at such a distance from the discs as made observation easiest for them; the distance was about 3m. The experiments were made with only one observer at a time, for periods of 50 minutes. Exposures were of 6 secs. duration, increased slightly for the trials where the greatest amounts of yellow were introduced, in order that the rate of introduction might be roughly the same. Instructions were as follows: "I am going to run a blue into a red on the color mixer. The change will be continuous. I want you to tell me whatever changes you notice in the color. The first changes will be very small, the changes in succeeding exposures increasing until you are sure of the change." The suggestion was generally, but not always, repeated just before each exposure: "Towards the blue," or "Still towards the blue."

### *Suggestion Group*

Observer *A* reports as follows with the following degrees of yellow present:  $10^\circ$ , "Barely possible it is darker;"  $20^\circ$ , "Perhaps slightly darker;"  $22^\circ$ , "Not at all certain whether the disc is a little bluish or not now. Quite certain of change but not sure of direction of change. Is it toward blue?";  $30^\circ$ , "I suspect all of a sudden that maybe there is blue in it and I've missed it before;"  $36^\circ$ , "Slight edge of color of rather greenish gray." *A* reported the following "prepossession": "*E* is working on suggestion, may give me no color at all, may give me wrong colors, may give me no changes, or wrong changes. May make movements intended to deceive me, also make time intervals wrong for deceit;" "Tendency to be careful;" tendency to make statements with "qualification, 'perhaps.'"

Observer *B* reports gray film, and says that it "may be due to the running in of gray." In the second of the two series *B* says, with  $8^\circ$  of yellow, "I guess there is a little blue in it;" and calls 6, 7, and 8 ( $12^\circ$ ,  $14^\circ$ , and  $16^\circ$ ) the same as 4 ( $8^\circ$  of yellow).

Among the reports of blue film by observer *C* occur the following:  $8^\circ$  of yellow, "Blue ring perceived and decided it was no illusion;"  $12^\circ$ , "Blue ring definitely present;"  $28^\circ$ , "Blue ring definitely present;"  $32^\circ$ , "Definite blue ring not so vivid as 14 [ $28^\circ$  of yellow] but more

than 15 [30°];" 32°, "Blue ring as faint as 15." From another series: 12° of yellow, "Obviously bluer;" 22°, "Much bluer than 1;" 28°, "Color has blue in it." After this series *C* was asked to indicate with discs how much blue he thought had been used, and he set them with 35° of blue exposed.

Observer *F* gave the following reports: with 18° of yellow, "Slightly more blue than 5 and 6;" 10°, "Noticeably more blue;" 12°, "Same as 5; perception of blue was less marked in appearance but lasted for a longer time;" 28°, "Perception of red changed distinctly towards blue; . . . surprised to see the blue so distinctly. . . . The blue present was very definite;" 34°, "Blue was very distinct, so much so that the color was quite blue;" 36°, "Blue lasted longer than in 15;" 42°, "The red seemed to be slightly darker, with a gray more than a blue."

Observer *G* was frankly suspicious after a few trials; her brother, she said, had 'fooled' her so often that she was generally on her guard. Some results were obtained: 8° of yellow, "Doubtful; first judgment 'darker,' second judgment 'lighter';" 14°, "Two judgments; one that the hue grew darker; second judgment 'no change';" 16°, "Changes from darker to lighter and darker again." *G* reported 'no change' or 'doubtful' for changes of 8°, 10°, 12°, 16°, 22°, 24°, 26°, 28° and 30°. At 28° she said: "Might have been a change towards blue."

Observer *H* reported the following: 10° of yellow, "Very faint blotches of slightly darker hue;" 10°, "Darker hue seemed to be in narrow bands;" 10°, "Slightly darker;" 10°, "Darker hued;" 18°, "A bluish-pink tinge in a filmy form, seeming to stand out from the background." Many reports of "No change" were made with the yellow shown at 10° to 40°.

In a few cases, *E* reversed the direction of stimulus-change at the end of a series, in order to avert a suspicion which had become evident in the observer's voice, manner or replies. In one series with *H* the results show that within a few trials a large increase of yellow and a large decrease of yellow can both be judged as changes in the same direction. In another series *H* reported "No change" when the stimulus-change was an increase of 34° and 40° yellow (14 and 17 steps beyond the DL).

Observer *I* frequently reported blue: 14° of yellow, "Medium blue discs . . . blue shadows;" 16°, "Single grayish blue disc rises over red from below and remains, not quite covering it;" 20°, "Red seems to blend with blue-gray;" 14°, "Bluish shadow appeared about a second after disc was exposed, . . . moved back and forth;" 18°, "Distinctly a violet-blue film over surface;" 20°, "Change to a purplish red with a great deal of gray in it, and this darker red, not a purple but a purplish red, . . . seemed to fuse this time except at edges and right in center of disc;" 22°, "Yes, grayish blue, . . . a little denser and a little darker and more mixed with the red than it had been before;" 22°, "Change to blue. It was unquestionably blue. . . . The blue was like a veil over the red, but it was unquestionably blue. It did not fuse with the red." Other reports of like nature were made.

Observer *J* reported the following: 10° of yellow, "Part . . . seemed a little darker . . .;" 28°, "Columns of shadow . . .;" 22°, "Columns of darkness;" 14°, "the orange-red changed to a brownish-red;" 30°, "Perception of a bluish-lavender, . . . perception of a more brownish-red background; the bluish-

lavender gradually changed to a more distinctly dark blue perception. The dark blue was not very definite, although I was certain that it was blue and no other color;" 32°, "A bluish-lavender appeared, 36°. . . I am certain that the perception was that of bluish-lavender;" 36°, "No change."

Further results of the same kind might be quoted from the reports of all observers. Needless to say, pauses were often made in the longer series, to avoid fatigue and eye-strain; the observers were continually cautioned beforehand not to make an observation unless entirely free from after-images; and the experimenter frequently enquired, during an experiment, about the presence of after-images.

### *Control Group*

Introspections were secured from observers *F*, *G*, *H*, *I* and *J*. We can give only brief summaries.

Observer *F* rarely reports imaginal processes; judgment comes "following directly the perception of the blue," "with the perception of blue as the clearest thing in consciousness." Once she reports a "readiness to say blue." There is apparently no change of attitude from the one group to the other; and assurance is no greater in the control than in the suggestion series.

Observer *G* expresses her judgments in "Yes," or "Positive," but a good deal of doubt is still reported. There is sometimes much imagery, expectant and memorial, and sometimes again this imagery precedes judgment; but ordinarily the judgment is reported as direct. There is change of attitude, but no marked increase of assurance.

Observer *H* has a good deal of imagery, and sometimes reports verbal processes, though both alike are usually "very dim," "merely an undercurrent of little importance." In recalling earlier series *H* says: "The changes to blue . . . in the suggestion series were clear perceptions, though not of so great change. . . . They were perceptions from the fact that what I saw was different from what I expected to see, i. e., a dark sector on red."

The shift from a suggestion series to a control series was made with observer *I* during one of the hour-periods. It was not, however, until 18° of blue were introduced that she said "Blue was not so blue in the other series." Her introspections show two kinds of judgment: in the one, there were many imaginal processes, in the other no imaginal processes at all were reported, and the judgment was touched off directly by the stimulus. Her assurance is the same in both groups. She used, in the first group, such words as "unquestionably," "distinctly," "clearly," in referring to changes toward blue, and showed no trace of doubt in tone of voice or in word. "As to the earlier group there was no question of the clearness of change and my sureness of the change. It was distinct, and a change, so far as I can tell, actually of sensation . . . ; the changes had all the characteristics of sensory experiences, and, at the time, I should not have thought of calling them anything else than sensory."

*J*'s judgments occasionally come with a "yes," and kinaesthetic-verbal elements are characteristic of her reports. In comparing the two groups she says: "No more clear or distinct than in the cases where I was sure of the blue;" "Experiences exactly like those in other experiments; things seem to go just the same."



*Summary of Results from Experiment 1*

(1) The records of the suggestion group show results that are very similar to (in some cases indistinguishable from) results reported in the control group. There are a few of these for A, B and G; many for C, F, H, I and J. Even where the observers think that they have found specific differences in the experiences of the two groups (as happened in certain instances with I and J), reference to the record shows that exactly the same things are reported in both sets of experiments.

(2) The assurance of F, H, I and J is the same for both groups; that of G is not markedly different.

(3) The introspections show (a) judgments with a good deal of imagery; (b) judgments which seem to indicate the touching-off of a tendency to agree with the suggestion, to say "Yes" or the like; and (c) direct judgments without imagery and with no apparent intervention of the motor impulse to accept the suggestion; here the observation itself appears to be modified. Judgments of the third or sensory type were largely predominant in this experiment with observers F and H, and were in the majority with G and I.

(4) The reversal of direction of large stimulus-changes, for two observers, is in the one case barely noticed and in the other, is accepted as a continuation of the series. Definitely supraliminal changes in opposite directions are, by both observers, judged as changes in the same direction.

*Experiment 2. Colorless Discs*

This experiment, made with black and white discs on the Marbe mixer, seemed fatally to arouse (or confirm) the suspicions of the observers. Hence E took only one series with A and five with C. In the series with A, the suggestion was "Towards brighter." With 6°, 8°, and 10° decrease of white A reported no change; with 12° he said: "Might have increased that time; can't be sure at all." The obvious suspicion of A made it necessary to discontinue the experiment. Observer C gave the judgment "equal," under the same suggestion, with decreases of 8° and 10°.

*Experiment 3. Colorless Light*

The Whipple box, set up in the dark room, was arranged to show light only in one window. The bar connected with the reflector was lengthened, so that small changes of stimulus were possible. Steps of 2° on either side of the neutral position of the reflector were used. The instructions were: "I shall give you a continuously changing light, and I want you to report as soon as you notice the change in brightness. Please indicate also the direction of the change. The change in the first trial will be very small, and will be increased in each succeeding trial until you notice the change." The suggestion

was repeated at the beginning of every series, and generally before every trial: "Towards the brighter," or "Towards the darker." Fifteen minutes were allowed for dark adaptation, and care was taken throughout the experiments to avoid after-images. The exposure was of 6 secs. duration, and was slightly increased for the larger changes.

### *Suggestion Group*

Observer *A*, with the suggestion, "Towards brighter" and the stimulus-change actually from  $0^{\circ}$  to  $12^{\circ}$  darker, reports: "Believe it did get lighter right near end." Again, with  $12^{\circ}$  change towards darker: "A wee mite lighter and then a wee mite darker, very small but *very definite*." With  $6^{\circ}$  and  $8^{\circ}$  darker *A* reported "lighter, doubtful;" and with  $10^{\circ}$  darker, "Wee mite lighter." It is to be noted that, just before this series, *A* had asked *E* to give a few practice trials in each direction, and that *E* had granted the request.

*B*'s reports frequently raise the question whether the change is as suggested. With  $8^{\circ}$ ,  $10^{\circ}$  and  $12^{\circ}$  change in both directions, "no change" and "doubtful" are reported. *B* was evidently suspicious; and in the last series, where the suggestion was "Towards darker," *E* reversed his direction. The change was from  $8^{\circ}$  in the one direction to  $12^{\circ}$  in the opposite direction, a change of  $20^{\circ}$ ; it was, however, accepted simply as a continuation of the series.

Observer *C* gives many "doubtful" and "no change" judgments, even where the stimulus-changes are clearly supraliminal. But he also gives inversions of judgment with full assurance. With the stimuli  $8^{\circ}$  and  $10^{\circ}$  lighter he reports "darker" emphatically. Again, with the stimulus  $12^{\circ}$  darker he says: "Brighter, sure." Once, when the stimulus-change was  $12^{\circ}$  darker, he reports: "Three periodic changes constituting increase of brightness, each followed by a darkening. That actually occurred subjectively."

Observer *F*, with suggestion "Towards brighter" and stimulus-change  $20^{\circ}$  darker, says: "Got brighter, distinctly brighter, then darker. . . . Change from lighter to darker was very slight." In another series, with suggestion "Towards brighter" and stimuli  $10^{\circ}$ ,  $12^{\circ}$ ,  $14^{\circ}$ ,  $16^{\circ}$ ,  $18^{\circ}$  and  $20^{\circ}$  darker, all judgments are "brighter." With  $18^{\circ}$ , e. g., *F* says: "There was a change from darker to brighter, quite distinct, short duration;" with  $20^{\circ}$ : "Much brighter than last, grew brighter for a time, and then a dark streak through center." Just before the second series of this experiment *F* asked: "Isn't there a good deal of suggestion here?" and added, "My natural tendency is to do the opposite." Immediately afterwards, with the suggestion "Towards darker" and actual changes of  $8^{\circ}$ ,  $12^{\circ}$  and  $14^{\circ}$  towards the lighter, *F* reported "darker," and said in every case that she was sure of the change.

Observer *G* was still on her guard. When the judgments were inverted, she was generally doubtful. With a stimulus-change  $22^{\circ}$  brighter, she reports: "That seemed darker; quite surprised at judgment; not very sure of it; seemed to get lighter at end; then more sure of judgment after I looked at it again; seemed an evident change." In another series, with a stimulus-change  $18^{\circ}$  brighter, she says: "Seemed as though getting a little darker; not sure; two or three judgments; first that it was darker; then that it was not darker; then that it was darker. Think it was imagination more than anything else."

Observer *H* gets definite results with full assurance. With suggestion "Towards darker" and stimulus-changes of  $10^\circ$ ,  $12^\circ$ ,  $14^\circ$ ,  $16^\circ$ ,  $18^\circ$ ,  $20^\circ$  and  $22^\circ$  towards lighter, all in one series, *H* reports that the changes are towards darker. In another series, "Brighter" is reported when the stimulus-changes are  $6^\circ$ ,  $8^\circ$ ,  $10^\circ$ ,  $12^\circ$ ,  $14^\circ$ ,  $16^\circ$  ( $18^\circ$ , "no change"),  $20^\circ$ ,  $22^\circ$ ,  $24^\circ$  and  $26^\circ$  darker.

The reports of *I* also show definite results. Characteristic is the following:  $18^\circ$  darker, "Change half way through; seemed to light up and a difference in depth; seemed to have third dimension, depth, as if I could put my finger into it; then stayed at this brighter intensity the rest of the time." Once when the stimulus-change is  $18^\circ$  brighter *I* reports changes in both directions, but says she is surer of the change towards darker. It is to be noted that this observer often reports twofold changes of this kind, and that she is always surer of the suggested changes than of those due to the actual change of stimulus. *I* even uses the word "brilliant" in describing a change with the stimulus  $20^\circ$  darker.

Observer *J*, with suggestion "Towards darker" and stimulus-change  $10^\circ$  lighter, reports: "Noticeable change first towards darker; constant for a while; then slightly brighter; change towards darker came suddenly, was distinct but only a slight change; change towards lighter was not quite as distinct; I was not as sure of that." With a change of  $22^\circ$  towards lighter she reports: "Distinctly towards darker; very distinctly noticeable; greater change than in any of the other experiments."

#### Control Group

Observer *A* reports an "agreement" which he "seems to have ahead of time," and he often replies with a "Yes;" "Judgment shot itself off, 'Yes.'" Sometimes *A* also reports a good deal of imagery. He is, however, an observer of a rather extremely 'objective' type, with a persistent tendency to hold fast to the stimulus and to accept the sense-impression as it comes. His judgments, so far as we can tell, are nearly always of the sensory type. There seems to be no greater assurance in this group than in the suggestion group.

Observer *B* often replies by "yes," and reports much kinaesthesia (eye-strain, tendency to nod head, etc.). Once he reports: "A vague memory of previous experiments when I had to be on my guard." His attitude and assurance were changed in the control group.

Observer *C* reports changes no greater or more definite in this than in the suggestion group. In fact we find an emphasis placed on a few, at least, of the inverted judgments which we do not find in the control group. *C* gives many imaginal and mixed judgments; but the sensory are the more numerous.

Observer *F* reports judgments with and without imagery. When asked to compare the experiences of this group with those of the suggestion group she said: "At the time they seemed to be actually sensations. There did not seem to be anything imaginal about them. I should probably have declared under oath that there was a change 'towards brighter' or whatever you suggested." After another trial she said: "Same effect though not as distinct as last time. This was about the same as in the first group. I don't think that if I had the two side by side I could have told any difference, unless this latter might be a little clearer and I can't be sure of that. They were very much alike, I am sure of that." And again: "In trying to decide whether my experiences were sensory or imaginal, there seemed to be both sensations and images, but I seemed to get things I did not expect

and had not imagined. Sometimes the light seemed to be brighter, so much so that I felt surprised. . . . Some of the results were different from that imagined, i. e., expected. It seems to me now that suggestion affected both imagery and sensations, but I can't tell which was affected most."

Observer *G* expresses much doubt in this group, but less than in the suggestion group. With 20° change towards brighter she is doubtful, and with 24° she says: "Guess that was brighter, not sure." After an observation with 20° darker, she remarks: "This seemed about the same as results in the suggestion group. I don't seem to notice any difference between this last experiment and the suggested experiences." Later, in support of her statement that her former judgments were due to 'imagination,' she says: "In sensation I see the change towards darker as a cloud. If it were imagination, it would be confined more to the window itself. When I shut my eyes I can get a clear-cut image of the window itself." But several reports in the suggestion-group refer to just this "cloud"-effect, which the observer maintains would make the experience sensory.

Observer *H* can find no difference in the experiences of the two groups. "Can't now pick out any differences. Changes seemed definite then and they are definite now." "The changes in the first group seemed just as much out in front of me; these in the second group no more clear or definite. . . . I think the change was sensory because the change came differently than I expected it would; I had images of what I expected, and what I saw was contradictory to the images." It should be noted that in this case the control group followed the suggestion group at an interval of only a few days.

Observer *I* agrees with *H*. "At the time I looked at them [the exposures of the first group] they were perfectly real; they looked just like a moving picture or panorama that moved in front of me; it was different from imagination and I did not doubt its objectivity." "They seemed like sensory experiences in that they seemed to be presented to the eye with visible changes. . . . There was also an . . . idea that the changes were not quite what I expected them to be; it was a little feeling of dissatisfaction, although I had no definite images of what I should get. . . . If you had said to me that it was just my imagination, I should have taken exception to it, because I really saw something there."

Observer *J* has slightly less assurance here than she had in the suggestion group. She reports that there is no difference that she can name between the experiences of the suggestion and of the control groups; "The effect of suggestion in earlier group was certainly, I think, on the sensations."

### *Summary of Results from Experiment 3*

(1) Again, similar (and in part indistinguishable) results are obtained from the experiments of the two groups.

(2) The assurance of *A*, *C*, *F*, *H*, *I*, and *J* is approximately the same in both groups. *I* expresses greater confidence in the suggested than in the actual change, when on certain occasions she reports change in both directions.

(3) The introspections again show three types of judgment, the imaginal, the motor, and the direct sensory judgment. The sensory type is the most frequent in this experiment: the judgments of ob-

servers *A*, *H*, *I* and *J* are predominantly direct; those of *C*, *F* and *G* are sensory or imaginal or mixed; those of *B* are largely motor.

(4) *A* finds a "very definite" change (judgment being inverted) immediately after *E* has given, at *A*'s request, special practice trials with knowledge. *B*, in the suggestion group, accepts a very large change of direction as simply a continuation of the series.

#### *Experiment 4. Visual Extent*

The Wundt apparatus was arranged to show a white rectangle, 14 by 60 mm., upon a black background. The rectangle could be lengthened or shortened at will; the unit of change was 1 mm. The observer saw the standard strip for 1 sec.; then came a pause of 5 secs.; and then the comparison strip for 1 sec. The instructions were: "I shall expose a rectangle, first, at the standard size; then I shall expose it at a length either greater or less than the standard. You are to note any change in length, and to indicate whether the second rectangle is longer or shorter than the standard. The change in the first pair will be very small, and will be increased in succeeding pairs till you notice the change." The suggestion was repeated before every trial: "Towards the longer," or "Towards the shorter." A head-rest brought the eyes of the observer 90 cm. from the frame. A gray card, placed just before the stimulus, aided fixation and also helped to eliminate after-images. Great care was taken to rule out secondary criteria. *A*, *B* and *C* took part in the experiment.

#### *Suggestion Group*

Observer *A* gave comparatively few inversions. With the variable 4 mm. larger, he reported, with no doubt at all, "shorter." With a change of 5 mm. he said "larger, doubtful;" with 6 mm. he was sure of "longer." In another trial a decrease of 3 mm. was judged "longer" with no doubt. There were many "doubtful" and "equal" judgments. *A* declared his suspicion of the use of suggestion almost at the beginning of the experiment, and said: "I seem to be more suggestible in this experiment than in the others."

Observer *B* showed in tone of voice and in manner that he questioned the nature of the experiment; and failure in four series led *E* to discontinue it. There were no inversions, but the judgments were mostly "equal" and "doubtful."

Observer *C* gave no inversions when the change was actually towards the smaller. There were, however, many "equal" and "doubtful" judgments with changes as great as 5 and 6 mm. *C* reports that, when *E* says the change will be in one direction, "the suggestion" seems to make it just the opposite.

#### *Control Group*

Observer *A* finds the "readiness" or "agreement" that he has reported before. The typical introspection shows little or no imagery.

He says, e. g., "Attention strong on second stimulus; 'shorter' came out immediately upon clear visual and kinaesthetic perception of the stimulus."

Observer *B* gives his judgment characteristically by "yes." "A peculiar auditory after-image immediately succeeded *E*'s voice, a sort of echo accompanied by verbal idea 'yes, smaller.' I not only say to myself 'smaller' but say to myself 'yes, smaller.' Sometimes I think it quite irrelevant whether I tell you my judgment or not. The fact that I say 'yes' to myself seems to show the working of the suggestion." "Strong repetition of instructions seemed to constitute the suggested consciousness. When I actually do see the second stimulus it is surprisingly more different than I expect. That surprise is mainly kinaesthetic and organic, partially in verbal comment as if setting of mouth to say 'why?'"

Despite the verbal set, *B* reports that there is no tendency to give a judgment before the second stimulus is perceived.

Observer *C* reports a good deal of imagery which appears to be largely responsible for the 'equals' and 'doubtfuls' of the suggestion group. "The perception of the white is not constant; the apperceptive process undergoes development and changes."

#### *Results of Experiment 4*

(1) This experiment was not successful. Only in one or two cases do we find inverted judgments given with an assurance as great as (or greater than) that of the correct judgments.

(2) The types of judgment—motor, imaginal, sensory and mixed—are fairly evenly represented. *A* and *B* tend here toward the motor, *C* is definitely of the imaginal type.

#### *Experiment 5. Noise Intensity*

The point marked 45 on the graduated arc of the Fechner pendulum was chosen as that from which to give the standard stimulus. The steps used for the variable were the divisions of the scale. The observers were instructed as in the former series. The interval between standard and variable was 8 sec. The suggestion was given before every trial; "Towards the louder," or "Towards the fainter." *A*, *B* and *C* took part in the experiment.

#### *Suggestion Group*

Observer *A* stated near the beginning of this experiment that he gave himself the counter-suggestion of equality of the stimuli; and *E* did not venture to give many supraliminal changes. With the suggestion "Towards louder" *A* reported: 38, "Weaker, very doubtful;" 42, "weaker, doubtful, just a trifle weaker;" 41, "equal or a trifle louder; second seemed to have more volume;" 40, "equal or a trifle louder." With the suggestion "Towards fainter" *A* responded: 47, "equal, doubtful;" 49, "louder, doubtful;" 50, "trifle louder;" 51, "weaker, not very much weaker, but my judgment was fairly sure." A reversal of stimulus-change, made at the end of one series, was accepted without comment, as if a continuation of the series. All of these changes were supraliminal.

Observer *B* reported: "Sometimes it seems to go opposite to what you say it will. I sometimes say 'doubtful' and wait till another step." With the suggestion "Towards louder" *B* replied: 42, "doubtful, a difference, not very sure whether louder or not;" 40, "equal;" 38, "equal;" 37, "certainly sounds less;" 36, "less." *E* then gave a large supraliminal change in the opposite direction, and *B* merely said "more," accepting the trial as a continuation of the series. With the suggestion "Towards fainter" *B* answers: 51, "about equal;" 50, "equal;" another 50, "That was less; that was quite surprisingly less;" 52, "That wasn't less, equal if anything;" 53, "equal;" 55, "equal;" and in this series it is only at 56 that *B* says "louder." *E* then altered his direction, and gave 40. *B* said: "Less, not as sure about it as other, but no doubt." This change, too, was accepted as a continuation of the series; although, just before it was begun, some practice trials had been made in both directions.

Observer *C* reports suspicion of the experiment. Yet with the suggestion "Towards louder" he responds: 42, "louder, sure judgment;" 36, "very slightly louder or equal;" 41, "louder;" 39, "louder" (*C* adds here that he could "judge confidently; my attention has been good, hence the confident tone of voice"); 38, "louder," again with confident voice. With the suggestion "Towards fainter," *C* gives at 51, "fainter, fairly confident."

#### *Control group*

Observer *A* gives his judgments nearly always by "yes" or "plainly" or some like word. In nearly every case the analysis of judgment shows the intervention of anticipatory auditory images. The situation differs from the preceding, in that now there is no self-suggestion of the equality of the two stimuli.

Observer *B* often refers to the automatic release of the response. "The word 'yes' was almost forced upon me, as if I could not say anything else."

Observer *C* also reports several cases in which the judgment was touched off automatically. He has, however, a good deal of imagery, visual and kinaesthetic, and once mentions an anticipatory image of sound.

#### *Results of Experiment 5*

(1) All three observers give inverted judgments with as much assurance as the judgments of the control group.

(2) Reversal of direction of the stimulus-change is accepted by *A* and *B* without surprise or comment. With both observers, the changes involved were large.

(3) The judgments in this experiment were characteristically of the motor type. With *B* these motor judgments are largely preponderant; and with *A* (who inclines strongly to the sensory) and *C* (who inclines to the imaginal) they are in the majority. *A* reports the almost constant appearance of auditory images.

#### *Experiment 6. Pitch*

The standard stimulus was a tone of 870 v. d. from the Stern variator; the unit of change, up and down, was 2 v. d. The duration of the stimulus was 6 sec., increased slightly for the largest changes. The observers sat about 5 m. from the apparatus. The instructions

were: "I am going to give you a continuously changing tone, and I want you to report as soon as you notice the change in pitch. Please indicate also the direction of the change. The change in the first trial will be very small, and will be increased in each succeeding trial until you notice the change." The suggestion was repeated before each trial: "Towards the higher," or "Towards the lower." Observers *A*, *B* and *C* took part in the experiment.

#### *Suggestion Group*

Observer *A* was so suspicious of the experiment that but few series could be taken. There was, however, one inversion of judgment with supraliminal stimulation.

Observer *B* was also very suspicious, and the series were therefore curtailed. There were, however, clear supraliminal inversions: e. g., with the suggestion "Towards higher" and the stimulus-change to 860 v. d., *B* judged "higher."

Observer *C* declared, before the end of this experiment, that he thought the experiments were on suggestion. *E* told him emphatically that he was making a study of limens, but that he did not wish to explain further. The few results obtained are the more striking as *E* gave *C* on request a number of trials for practice, both up and down, at the beginning of the series. Most of the inversions occurred at 6 v. d.; but once, with the change of 8 v. d. higher, *C* replied "down" with no comment; and once, with the change of 8 v. d. lower, he replied "higher" with no comment.

#### *Control Group*

Observer *A* finds nothing new in introspection except the prevalence of tonal imagery.

Observer *B* reports characteristically by "yes" or some like word. Once he says: "My readiness to make the judgment was present before the tone came."

Observer *C* still reports much imagery.

#### *Results of Experiment 6*

(1) This experiment was not successful. A few inverted judgments, especially with observer *C*, show an assurance equal to that of the control group; but the results are very scanty.

(2) *B* gives judgments chiefly of the motor, *C* of the imaginal or mixed type. *A*, as in the preceding experiments, is of the sensory type.

#### *Experiment 7. Noise*

Series of 10 stimuli were given by the modified acoumeter; 8 with the larger lead ball commonly used; 2 (at varying places in the series) with a cotton ball made compact by a few stitches of thread and blackened with ink. Preliminary observations were taken with the lead ball, in order that this sound might, in the experiments proper, be made definitely supraliminal. The observers were seated



at a distance of 3.5 to 4.5 m., from which they could easily see the ball drop. The intervals between trials were of 8 sec. duration. The instructions were: "I want to make a few preliminary trials to find the limen roughly and see if I can make use of the experiment. Tell me whether or not you hear the sound from the ball dropping. You are to watch the ball, and to report after you see it drop." The suggestion lay in the fall of the cotton ball. Observers *A*, *B*, *C*, *G*, *I* and *J* took part in the experiment.

#### *Suggestion Group*

Observer *A* expressed surprise that he saw the ball drop but did not hear it. Once he "heard it hit the side after it dropped;" and once he replied confidently with "Yes." A little later he said he believed *E* was using two balls and trying to 'fool' him.

Observer *B* reported hearing the cotton ball three times; once he gave the judgment in a "yes" that indicated conviction. During one series he remarked: "When I don't hear it I think it is a lapse of attention; it takes about all the attention you have."

Observer *C* often reported that he heard the ball drop. His reports were given with full assurance, notwithstanding the fact that he several times failed to hear the ball and complained of lapses of attention.

Observer *G* reported 'hearing' the ball drop once; *I*, once; and *J* not at all. Failure was due, in part at least, to suspicions on the part of the observers. *G* expressed some doubt about her judgment, although she called the experience a "perception of sound;" and her final judgment was "positive."

#### *Control Group*

No introspections were taken from *G* and *J*. The two following reports from *I* are typical.

Lead ball used: "Visual perception of falling shot; fell in straight line. Auditory perception: sound of falling shot; quality clear; auditory image of ticking of clock. Duration very short; sound faint and far away."

Cotton ball used: "Visual perception of falling shot. Seemed to fall faster than before. Auditory perception, sound of shot falling, clear; duration longer than before. Sound quite loud and unmistakable; visual and auditory image of dried pea falling on glass counter."

#### *Results of Experiment 7*

We have found cases—a considerable number with one observer—in which a cotton ball is 'heard' to drop just after a lead ball has given a definitely supraliminal stimulus. The judgments are passed with assurance; and they are not corrected later, although the true stimulus is immediately repeated. There is nothing to show that the suggested experience could be distinguished from that aroused by adequate stimulus.

*Experiment 8. Smell*

Two paper funnels projected from a small closet in which *E* sat during this experiment. *E* often made various noises to suggest the presence of apparatus; no odors were used. The observer sat about 3 m. away, directly in front of the funnels. The instructions were: "I shall give you either a pleasant or an unpleasant odor, which will come from one of the funnels, and I want you to tell me as soon as you notice it. I shall keep the time with a stop-watch. As soon as you report the first odor, I shall give you another which will neutralize the first, and I want you to tell me as soon as the first odor has disappeared." The suggestion was made before each trial: "I shall now give you a pleasant [or unpleasant] odor." The observer was asked to give as full an introspection as possible after each successful trial. Observers *D* and *E* took part in the experiment.

Observer *D*; suggestion of "unpleasant odor." Duration, 31 sec.: "Got just one whiff of unpleasant odor; it smelled like hydrogen disulfid. I had a decidedly unpleasant odor." Duration, 106 sec.: "Before getting the smell there was a watering of the eyes, then rapidly followed a tickling in the nose and in about the same interval the smell came. I tried hard to get smell all through period; concentrated attention to get it. It does not seem to stay or grow more intense but comes and goes. The odor was very clear; I do not believe it could be any clearer. It burst out quickly; got no after-image. My eyes smart a little now." Duration, 97 sec.: "Same characteristic tightening in forehead and sensations in throat. The smell is the same I have spoken of only more intense. It was disagreeable and I smelled it a long time before it was disagreeable; not very clear at any time, but got the odor definitely; what I reported I got." Only two positive results came with the suggestion of "pleasant odor." Once *D* said: "I can't really smell it, but think there is a smell there because of the change in blood pressure;" and again: "Slight odor; no pleasantness or unpleasantness."

Observer *E*: Suggestion of "unpleasant odor."—Duration, 96 sec.: "It was rather irritating; seemed to irritate lining of nose and throat; something like ammonia. It wasn't intense enough to be really unpleasant. I could only get it by keeping my attention on it. Fairly clear." Duration, 97 sec.: "The unpleasant odor seemed fainter than before, and I was rather conscious of smelling something than of a particular smell. It was irritating. It was not clear."—Suggestion of "pleasant odor." Duration, 56 sec.: "Very faint and of a pleasant soothing nature. I got it as an odor only, that is there was no irritating effect, and I got it in the fore part of my nose and it was fairly clear." Duration, 134 sec.: "It was pleasant, of a soothing, sweet nature; I got it in the fore part of my nose. It was rather clear and lasted about three minutes."

*Results of Experiment 8*

- (1) The reports of odors are given with full assurance.
- (2) Although the reports mention various modes of tactual experience, yet they distinguish these from the experience of smell proper.

*Experiment 9. Taste and Smell*

Specially prepared small candies were used. Half of them were wrapped at the time of the experiment in tin foil; these were also of different shape from the unwrapped. All were made at the same time, and were of the same material and the same size. The foil-covered candies were called A, the others B. The observer tasted A (or B) for 15 sec.; then rinsed the mouth (about 30 sec.); then tested B (or A) for 15 sec.; and then gave his judgment. The instructions were: "I am going to give you two stimuli, and I want you to make a careful comparison between the two as regards gustatory and olfactory sensations." A, B and C took part in the experiment. Each observer made 10 comparisons. Differences were reported as follows:

Observer A: A sweeter 6; B sweeter 2; A more bitter 4; B more bitter 1; A more olfactory 4; B more olfactory 1; A smoother 2.

Observer B: A sweeter 1; B sweeter 6; A more bitter 4; B more bitter 1; A more sticky 6.

Observer C: A sweeter 2; B sweeter 3; A more bitter 4; A more olfactory 2.

The following are typical introspections. Observer A: "In A the sweet tasted first quite intense, then bitter, with smell quite intense. Intensive changes or alternations. A also is harder than B. In B the sweet is less intense than in A. Bitter is a long time coming as also is the smell, neither so strong as in A. No such alternation of large-small intensity as in A." Observer B: "A called forth first tactual and vague kinaesthetic sensations, sweet, not strong, some slight cold sensations in middle of tongue. Finally a bitter almond taste, not quite as strong as the sweet. Touch outlasted them all. B was very much sweeter, sticky, and the last burnt smell came only immediately before rinsing the mouth. The kinaesthetic sensations from chewing were somewhat clearer than in A, but not more intense." Observer C: "B held at the tip of tongue. At first a very bitter sensation there. Then the stimulus was broken up. Now sweet sensations for a considerable period mild, then becoming very strong with a very faint admixture of bitter sensations. A held at tip of tongue. Stronger and more widely distributed bitter sensations than with B. Then the stimulus was broken up. Now sweet sensations for a time mild, then becoming seemingly as strong as those of B. But, perhaps, not maintaining this high intensity for so long a time. Admixture of stronger bitter sensations than with A."

At the close of the experiments the observers were asked if they had noticed any constant differences between A and B. Observer A decided that A was generally smoother, softer and sweeter; quite often more bitter and having a stronger smell than B; taste and smell sensa-

tions rise more quickly in A than in B. (His reports show that A is generally reported as sweeter, more bitter and more olfactory than B; B also is reported as harder than A, and A as smoother than B.)

Observer B decided that B was sweeter and more sticky than A. (This agrees with the reports, which show that B was sweeter and more sticky than A; B is also reported, however, as more bitter.)

Observer C decided that A and B were the same, except that A was the more bitter. (This excess of bitter is the difference most prominent in his reports.)

#### *Results of Experiment 9*

(1) The reports of difference are given with full assurance, and differences are often reported as large, although the observers, especially A, had their suspicions that the stimuli were identical.

(2) The judgments are all (except in one instance with observer C) of the direct sensory type.

#### *Experiment 10. Pressure*

Stimuli were applied to the back of the hand by the ivory button of the Wundt apparatus; 50 gr. was the standard; change was made by steps of 2 gr. Application was for 1 sec., and the interval was 5 sec. Care was taken to prevent fatigue. Instructions were as in other experiments. The suggestion was repeated before each trial: "Towards the lighter," or "Towards the heavier." Observers D and E took part in the experiment.

#### *Suggestion Group*

D gives many "equal" judgments. With suggestion "Towards heavier," and lighter stimuli 6 (once), 12 (once), and 8 (several times), he says "heavier." Twice E reversed the direction of stimulus-change, to avoid suspicion: in the one case, in which the second stimulus was lighter, D said "heavier" at 6, and "equal" at 8 to 16; change was then made from 16 lighter to 8 heavier, and D accepted the stimulus as a continuation of the series, and still judged "equal."—With suggestion "Towards lighter" there were no inversions, but many judgments of "equal."

Observer E reported "heavier" when the stimulus was 8 lighter (twice) and 10 lighter (twice). There were no inversions with the suggestion "Towards lighter," though there were several "equal" judgments.

#### *Control Group*

Observer D reports a great deal of imagery, visual, verbal-auditory, and anticipatory tactual. Thus: "Sensation from first stimulus; images left consciousness entirely, tactual sensation very clear. Then tactual image came, and verbal-auditory image of word 'strong.'"

Observer E also describes much imagery, chiefly visual.

#### *Results of Experiment 10*

This experiment was inconclusive. Several inversions, however, occur with full assurance. Reversal of direction of stimulus-change was possible without comment from the observer. Judgments were predominantly of the imaginal type.

Practice experiments made with observer *F* and another whom we shall call *K* (a man taking advanced work in psychology) led to results that may be mentioned here. Discrete stimuli were given with a small spring pressure-apparatus, and no misleading suggestion was employed. The observers were told only that two pressures would be given, the second heavier than the first, and were asked to make as full introspections as possible.

Observer *F* reported: "First noticed the instrument before it touched my hand; then felt pressure and tried to carry it over in memory; held breath till second came. Second more intense; slight pain; seemed to feel it coming by degrees till instrument touched hand." *E* asked *F* to say more about the fore-period, before the stimulus was applied. "First feeling was less intense. It seemed to last longer than the pressure itself, quite a little longer. It was clear and definite, but much less intense than the pressure; seemed to get nearer by jerks. Just as clear as actual pressure, only less intense. Remember surprise at feeling it before stimulus touched my hand." And again: "First felt pressure near third finger; then felt touch from first stimulus, much less intense than pressures in other experiments and not lasting so long. . . . The first [pressure] before the stimulus touched the hand . . . was like a big surface; seemed heavier than the two following [from stimuli], not so clear cut; other two real ones seemed less intense than the first. . . . All three seemed about the same in clearness. The first, i. e., the one before the stimulus, seemed to be of longest duration. The two from the stimuli seemed to have smaller area."

Observer *K* reported: "Auditory perception of *E*'s voice. Some imagination, imagining what feeling would be like. Really, a tactual sensation before the stimulus was applied; rather diffuse, seemed much lighter, smaller, to cover less space than perception with stimulus. The real perception much clearer; following the perception was an after-image." *K* added that the 'imagination' of sensation before the stimulus was applied "would be as clear as a very light sensation produced by the instrument."

In these reports we have a mention of anticipatory 'images' which seem closely to simulate sensation.

### *Experiment II. Temperature*

Cylinders 1 cm. in diameter were used; 35° C. was the standard stimulus, and the variable increased or decreased by 1° in successive trials. Instructions were as usual; the suggestion was repeated before every trial: "Towards the warmer," or "Towards the colder." The stimulus was applied for 2 sec.; the interval between stimuli was 5 sec. The inner side of the fore-arm was stimulated. Observers *A*, *B* and *C* took part in the experiment.

*Suggestion Group*

Observer *A* said after a few trials that he thought the stimuli of each pair were probably the same. However, with the suggestion "Towards colder" he judges 4° warmer as "colder;" 5° warmer as "equal;" and in another series, 3° warmer as "colder, doubtful." There were no supraliminal inversions when the second stimulus was colder.

Observer *B* with suggestion "Towards warmer" gives one inversion, at 2° colder. With suggestion "Towards colder" he judges 3° warmer as "colder" (twice), as "slightly colder" and as "equal"

Observer *C* with suggestion "Towards warmer" misjudges only once; 2° colder is called "warmer." With suggestion "Towards colder" he calls 3° warmer, "very decidedly colder;" 4° warmer, "very slightly colder or equal;" 6°, "equal;" 7°, "colder." After this judgment *C* said: "These judgments are more marked, the judgments more confident, than in the case of the visual sensations."

*Control Group*

Observer *A* several times reports anticipatory images. "*E*'s voice . . . in background; but was sufficient to arouse visual and tactual image of coming stimulus." Again: "Tactual image of how the stimulus would feel." Again: "*E*'s 'ready' and 'now' though heard inattentively caused increased strains and anticipatory tactual image." Once he speaks of a "tactual and cold(?) image."

After one of these reports, *E* asked *A* why he used the words "image" and "sensation" as he did. *A* replied: "If I had used the word 'sensation,' that would have meant that you touched me or that I thought you did. A touch sensation is different from a tactual image. I should say there is generally more of a sensation. I mean that the image is abbreviated, does not contain all that the sensation contains; perhaps, is of less extent, less intensity, generally, and I think (keeping this experiment in mind) that its extent is less clearly defined." After another experiment he remarked: "The criterion is probably the source of the stimulus, though why I answered as I did was probably unconscious."

Observer *B* reports: "Here I had a good anticipatory cold image, localized on the fore-arm before the stimulus was given. Verbal comment in imaginal terms: 'Why, here I am feeling cold.' Attitude of surprise." Asked why he used the word "image," *B* replied: "Might have been 'sensation' if the tactual sensations had also been present." In another trial *B* says: "Slight pressure and distinct warm sensations from the time *E* said 'now' up to the giving of the second stimulus." Here he uses the word "sensation."

Observer *C* reports "a diffuse cutaneous image of warmth in a certain cutaneous region of the forearm. Under the influence of the *Aufgabe* this became more intense." Again: "In mid-period kin-aesthetic-auditory images 'the next will be warmer;' then attitudinal process meaning roughly 'It is really so;' then cutaneous image of warmth, warmer than first [sensation] but not so warm as second proved to be; feeling of surprise." In differentiating between "sensation" and "image" at the time of these accounts, *C* said that, in the case of the sensation, there were other intermingled sensations. There was also a different attentional accommodation; the sensation was more intense and compact; the image was, as a rule, less intense.

*Results of Experiment II*

(1) The suggestion group shows inversions of judgment for all three observers; the judgments were made with full assurance, and in the face of some suspicion as to the nature of the experiments.

(2) The various types of judgment—motor, imaginal, sensory and mixed—are in this experiment fairly evenly represented.

(3) Anticipatory "images" of fair intensity play a part in the judgments.

## E. DISCUSSION AND CONCLUSIONS

We have relied, in this paper, upon the reports of trained observers; and we have been obliged, in consequence, to utilize the observers *A*, *B* and *C* for experiments upon which they entered with definite suspicion. The effects of the verbal suggestions were thus, we may suppose, markedly lessened. Unless, however, we had the analyses given by these observers, we could hardly place any high degree of confidence in the positive statements of the less trained observers; types of judgment must be distinguished before interpretation is possible. It is regrettable that the limits of space have prevented our quoting in full some of the elaborate reports of the Control Group; still, enough has probably been given to indicate the general nature of the work.

Our first difficulty, then, lay in the necessity of recourse to trained observers. These men, just because they were trained, were less suggestible than the average and were likely to suspect the character of the experiments. A second difficulty arose from the necessity of prolonging the single observations. If a continuously changing color is to be wrongly 'seen,' or if a second noise is to be wrongly 'heard' in relation to a preceding noise, then the color must not change too quickly and the sounds must not occur in too quick succession. Our durations and intervals were chosen, after preliminary experiments, as times that should be long enough for the working of the suggestion and yet not so long as to preclude the possibility of a direct sensory judgment. We cannot pretend to have made them optimal; further work is desirable, with variation of times, just as further work is desirable with more trained observers and with other modes of stimulation.

Our positive results may now be summed up as follows:

(1) We have found three types of suggested consciousness, yielding three typically different kinds of judgment. (a) In some cases we note a carriage of the verbal suggestion, as if across the sensory contents, to the far end of the arc, i. e. to the motor expression of judgment. If it is too much to say that the sensory contents in such cases are irrelevant, we can

at least affirm that rather startling approximations to that state of affairs have occurred in the course of the experiments. The judgments of *B*, in particular, are largely of this type; they seem to represent the ripening of a motor impulse; they 'go off in a Yes' or 'say themselves.' It is hardly questionable that many instances of 'sensory suggestion' are, in reality, of this *motor* sort. (*b*) In other cases we note a free play of relevant imagery: of imagery that is largely due, in all probability, to the long-drawn-out character of the observations. The imagery clearly serves, in many instances, to furnish a new context or setting to the presented stimulus, and thus influences or determines judgment; in other words, the judgment expresses an imaginally modified situation, and does not bear directly upon the stimuli. Fully half of *C*'s judgments, and rather more than half of *E*'s, are of this *imaginal* sort. (*c*) In a third or *sensory* type of judgment, imagery is either lacking or fleeting and apparently irrelevant, while there is no indication of any motor impulse to express the suggestion. Judgments of this kind are the most frequent with the remaining observers and form (so far as we can judge) just over 40 per cent. of the total number of judgments passed in the Control Group. They alone can furnish evidence of a truly 'sensory' suggestion. (*d*) Lastly, there are judgments which cannot be placed outright under any one of these three rubrics, and that must therefore be classed as *mixed*. The combination imaginal-sensory is fairly common, and observer *A* gives a good number of judgments in which the sensory and the motor determinations are both involved.

(2) We have found sensory suggestion, vouched for by sensory judgments, with color and light (continuous change), with 'liminal' noise, with smell, taste and temperature. Our experiments with grey discs and tonal pitch (continuous change) were unsuccessful, mainly, it appears, by reason of the suspicion of the observers; those with visual extents failed, partly for the same reason and partly because the experiment favored other than sensory judgments; and those with noise intensities and pressures were inconclusive, because the experiments favored respectively motor and imaginal judgments. Other observers and other experimental arrangements might, of course, yield positive results. We have found, also, 'images' of touch, warmth and cold which simulate the corresponding sensations.

We do not hesitate to draw the conclusion that, in certain departments of sense, a verbal suggestion may arouse conscious processes which are, phenomenologically, identical with those



ordinarily aroused by an adequate stimulus or change of stimulus. The departments are those of sight, smell, taste and temperature. We think it probable that hearing and touch may be added to this list; but for those senses our evidence is incomplete or inconclusive. We have already pointed out, on the other hand, that many of the illusions ascribed to sensory suggestion by previous writers are of doubtful character. No observation may be put down as a case of sensory suggestion unless (a) the type of judgment is known to be sensory, and (b) changes of the sense-organ (adaptation, after-image, etc.) have been prevented by a fitting control of the experimental procedure.

We do not offer any physiological theory of our results, nor do we enter here upon the question of the relation of 'sensation' to 'image.' It would be premature to attempt a theory (or theories) on the basis of the present observational material; we still do not know, e. g., the qualitative range of color suggestion, or whether Baldwin's "red for green" is possible.

(3) No proof was needed that observers are 'suggestible.' We were, however, surprised at the potency of suggestion in the case of observers who had frankly avowed suspicion. *A* and *C*, the one unusually 'objective' and the other markedly 'subjective' in type, would both, again and again, 'accept' a suggestion against which they were, in general, on guard. We have here, then, a striking confirmation of a principle that of late years has won its way to recognition: the principle that the experimenter must exercise great care in the phrasing of his instructions, and must not be wearied of repeating them.

If we count the inverted judgments in the series which were taken both by men and by women, we find that the women have an excess of 3 per cent. In view of the lesser training of the women, we regard this difference as negligible.

## PSYCHOANALYSIS AND THE STUDY OF CHILDREN AND YOUTH\*

By DR. O. PFISTER, Pastor in Zurich  
Translated by Frederick M. Smith, Clark University

By psychoanalysis is understood a method established by Prof. Sigmund Freud of Vienna, which without resorting to hypnosis investigates the unconscious contents and motive forces of the mind. That such psychical powers rule below the threshold of consciousness cannot to-day be denied. Great poets and other students of human nature have long asserted that the most exalted achievements of the human mind have sprung up out of the unconscious. Exact psychologists like Janet, James, Forel, Flournoy, nay, experimental psychologists like Ach and Offner, have demonstrated conclusively the presence of formative forces beyond consciousness. The only question is, what rôle do they play? Shall we compare them, to recall a well-known children's game, to the magnet rotating in a canister, which causes the figures placed on top to dance? Or conversely does the unconscious depend primarily upon the conscious, and is it similar only to the dragging anchor which checks the headway of the ship? Or has the unconscious the significance of land or fresh water which originally rose from the ocean as vapor and then fell from the heights, in part to nourish unseen the roots, in part to reappear in changed form as springs? No comparison is fully suitable to the facts. If I prefer to compare that unconscious with which psychoanalysis is concerned to the land or fresh water it is for these special reasons: the conscious is no more a mere puppet, as several analysts formerly believed, than are free flowing currents without effect. But the unconsciousness is not merely ballast, as others have declared, but a mighty, formative, psychic reality which influences the conscious as much as it is influenced by the conscious.

Can it really be possible to descend into the realm of this earth spirit? Psychoanalysis promises that we may do so. By what means this is accomplished can here only be sug-

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gested; it subjects the phenomenon to be analyzed to sharp scrutiny and gathers all kinds of fancies which apparently make their appearance fortuitously in this connection. These associations yield a conglomerate which upon a more exact examination can be explained as a significant whole, just as one classifies portions of an exhumed skeleton as parts of an organism.

When I assert that the unconscious is the goal of psychoanalysis and that its technique is the collection of associated fancies to an apperceived structure as well as the interpretation of these fancies, then I believe I have named fully the characteristics of its concept.

Already psychoanalytical work has taken such a range that a brief review must limit itself to mere allusions. First I should like to express my sentiments concerning the objects of psychoanalysis.

### I. THE OBJECTS OF PSYCHOANALYSIS

At first only adults were considered whose mental or bodily derangements reverted to psychical causes. But thereby one was led to occurrences or even mere fantasies which are connected with the earliest childhood up to the age of perhaps four years. In no adult were these infantile roots lacking. Thus even the study of the adult became child study. Then gradually was awakened the courage to subject afflicted children to serious analytical treatment, as pedagogical scruples had till then stood in the way of the direct examination of healthy children. The few child analyses, those other than pedagogical and medical, brought equally favorable results (grave phenomena of anxiety were under investigation), which essentially confirmed the conclusions as to the psychical life of children which had been drawn from the examination of adults.

That we might pass from gray theory to the green meadow of direct perception let us be reminded that even analysis in the beginning turned itself to the individual symptoms alone. I first select a paradigm that may by virtue of its simplicity and lucidity gain the favor of the hearer. A boy is one morning dumb, and later in the forenoon sees everything enveloped in deep darkness and collapses in the effort to arise, while he senses a line of pressure across the breast. From a single subsequent investigation it developed that on the previous evening he had been about to confess a dereliction to the mother. Prevented in this by feelings of shame, he said to himself dejectedly: "I cannot even speak as I will! Now it

becomes completely dark about me! I hang merely by a thread!" One sees in a moment that the hysterical symptoms of dumbness, the dimness of vision, and the tension upon the breast only express symbolically the thoughts of the previous evening. They are, then, the expression of an autosuggestion, but they serve also the unacknowledged wish to escape a burdensome duty, for the dumb person can indeed make no confession, the half-blind cannot clearly see his task, the one hanging by a thread cannot firmly tread the ground. But this intention (and this is especially to be emphasized) was totally foreign to consciousness.

It may seem peculiar to the novice, yet the stubborn facts force the admission, that all psychoneuroses without exception form illustrations of such unconscious wish introduction. Impulse emotions lie at the base of them, and where an impulse neither reaches its goal nor is eliminated, a wish always comes into existence. It may be added that in this connection this word is used in a somewhat broad sense. Psychoneuroses are the picture and puzzle book that the unconscious holds before the observer. And what a gigantic picture book it is! We think of the host of cramps (e. g. hysterical screaming, writer's cramp, hysterical laughter, hysterical weeping, stomach cramps), of megrims, bowel troubles, hallucinations, imperative ideas, imperative feelings, imperative actions, convulsions, etc., we think of the tremendous effect these phenomena exert upon humanity, and we truly acquire respect for the great charade museum of the unconscious.

Now while Freud's incomparable sagacity followed the juggling of the unconscious he made a new and singular discovery. Among the smuggler-wares of the unconscious were found many of the same articles that even the healthy carry around with them for the most part neglected as worthless. Among such is the dream, which is indeed an enigmatical picture book. Among these contrabands we must also number the errors, such as striking errors in speaking, errors in writing, errors in seizing, forgetting, etc. Finally it was found that even in intentionally meaningless actions, figures, drawings, performed without purpose the unconscious ruled. What is meaningless for the psychology of the conscious proves a shining instance to the unconscious, at the same time being biologically purposive.

Had the neurologist Freud become an investigator of childhood and of normal psychology he could not have entirely turned aside from the study of the whole individual. Even the pathological symptoms, apparently lying ever so far apart,

were most closely connected and issued from conflicts in the center of personality. Was it, then, not a natural next step to investigate the effects of those unconscious processes upon the whole life's structure which psychoanalysis made clear? The problems of the study of youth were greatly enriched. A multitude of phenomena of youth that had created wonder as absolute riddles could now be understood. One understood kleptomania, pathological lies, strange imperative actions, as e. g., counting in walking, the anxious appeal to oracles, color-hearing, morbid disinclination towards special topics in school, sexual perversity, inexplicable love towards totally unfit persons, or inability to love parents or other people, fanatical devotion to foolish religious forms, pessimistic melancholy, sullen anarchy, and a thousand other facts that from the standpoint of traditional psychology had been completely inexplicable. Though many of these studies go a step past child study yet they are quite significant for the latter because in the child age the seeds are sown which later dominate the whole program of life. Out of infantile connections, of which we shall soon speak, issue the disturbing inferiorities but also incidentally the magnificent highest achievements which had previously formed the impenetrable secret of psychology.

## II. THE ORIGIN OF THE UNCONSCIOUS POWERS

Insufficient as were my somewhat arbitrary suggestions concerning the sphere of endeavor in the analysis of youth, yet it is to be hoped that they created the impression that we have to deal here with important psychological problems, even if the pedagogical result were not present at all, which to the practical paidanalyst is the chief thing. Now, however, I presume upon the healthy skepticism of my hearers and their avidity for facts and hence expect the question, Can, then, the subliminal forces whose supreme influence upon life and its highest accomplishments will not be denied, be recognized with surety and clearness?

Only proper observations can decide. Whoever is satisfied with the fact that a significant number of noted learned men of various countries found the essential theses of Freud confirmed, is so at some risk. To-day one cannot well depend upon the indolent excuse that psychoanalysis cannot be learned and that its affirmations cannot be controlled by experiment. Whoever is not too indolent or too faint-hearted in the face of prevailing theories, and who understands in general how to observe himself and to form theories, can after some trials institute some very good psychoanalytical experiments. What

I propound in the following will therefore be an invitation to the hearer to study fundamentally the psychoanalytical method in order to demonstrate and to supplement the statements here made.<sup>1</sup>

Psychoanalysis, as well as traditional psychology, acknowledges inborn impulses, instincts, psychical dispositions. It commits to others the task of determining the origin of these psychical realities. It acknowledges also the forgotten or unobserved mental contents which compose the potential inventory of memory of the components of perception. In all events which are germane to it it concerns itself with an unconsciousness of another kind, with highly worthy ideas and endeavors that in part were conscious but through opposing ideas and endeavors are suppressed from the conscious as painful, and in part through the same antagonistic powers are repressed and prevented from becoming conscious. In both exclusions for consciousness the unconscious can be disclosed only by its effects; but this is done, it seems to me, with the same surety that I draw conclusions from the psychical happenings of other men, although I cannot disclose them directly. Should it be a matter of repression or a holding aloof from the conscious, the unconscious, which the analysis first searches out, is to the conscious mental processes a concealed combatant who from his invisible lurking place often exercises a far greater power than if he had maintained his place in the conscious.

As an example of such repression I mentioned the dumb, half-blind, and hanging boy who in the moment of sickness had no recollection of the thought suggesting those symptoms and the wish lying behind them. Striking are the examples where a violent suffering of a decade's duration is generally known to go back to such a presentation. In the light of such experiences one understands very well how in the beginnings of psychoanalysis it could be said that the psychoneurotic suffers from reminiscences that lodge in the unconscious like a sort of foreign body.

Every repression means a further checking of development, a fixation. The impulse which is repressed by a definite achievement, a concept, a feeling, or an action, does not again venture out openly. It is the history of the pike in the carp reservoir. A glass partition was placed between the pike and the carp, and as the greedy fish dashed upon the intended prey it smashed its nose most painfully. After that it avoided its neighbor, even when the invisible barrier was removed. One

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<sup>1</sup> I refer for the rest to my book, "Die psychanalytische Methode," published by Klinkhardt, Leipzig, 1913.

also sees in this comparison that in which the task of the analyst consists: he ought to show that the partition does not exist or that it can be overcome. It is significant for the study of youth that even in the first years of life associations are formed in consequence of repression, associations which can, not only in the tenderest years, create neurosis, but very often disturb the whole future life, as a gnarl mars the growth of the plant. The mental life can never be conceived as immobile, perhaps not even in the severest sickness. The progressive development influences repression and fixation by impelling the impulse along a path which it otherwise would not adopt, where it conforms little to the interest or natural desire of the individual. In this progressive development, then, new formations are consummated whose final result we meet in artistic, religious, ethical inspirations, as also in the little errors of every day or the significant, often surprising fictions of new disease symptoms. So far as these phenomena indicate a departure from the original course of development we can consider them the product of expulsion.<sup>2</sup>

This expulsion may occur in the most various ways: In the formation of strange concepts chiefly of symbolical art, in the transfer of the feeling energies to entirely different spheres of life; e. g. of love to mathematics or philosophy or religion or politics. The whole "signature" of the character can be very strongly modified, although the original nature of man always has a very important word to say about this. The criminal of lost honor, the fanatic for truth of repressed mendacity, the fanatic for purity who has to fight with unclean desires, the heresy hunter who conquered cruel desires against comrades or animals, are named as simple illustrations. That psychoanalysis can obviate such repulsions does not more closely concern us here. We see however how the repression of a single content is followed by a holding off of other psychical processes from the conscious, namely, even those remodelling ones whose result consciousness sees in the oft-mentioned pathological and inspired achievements.

Now how shall we think of the ideas and endeavors which fall to the lot of repression and its consequences? This equally knotty and noteworthy question leads us to one of the points upon which in analytical circles a heated battle has raged. There are in substance three opposing views. The first was

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<sup>2</sup> The teachings of the church asserted rightly as the presupposition of Christian devotion the overcoming of the "natural man," only that the goal itself is not unnatural but is the expression of the "higher nature" of man.

originated by Freud, and as above mentioned it is the sexual impulse which after its repression brings about all psycho-neuroses and the other evils of repression mentioned. To clearly understand this proposition the reader must accustom himself to a careful consideration of the Freudian terminology and sex theory; otherwise he may find himself in a horrible morass and will, as many incautious critics have done, accuse Freud of having lured him there. But who is able in a few minutes to present this bold, complicated, and paradoxical sexual theory, without engendering real misunderstanding? Only a few oft-mistaken traits may be mentioned: Freud, according to his express explanation, used the word *sexuality* in the enlarged sense of love generally. He added further that sexual desire in the narrow sense enters into higher functions, as sympathy, friendship, art, religion, and can thereby lose completely its original sensual character; also ambition, striving for power, and similar egotistical motives can be augmented by sexuality. Unfortunately it is furthermore not superfluous, certain opponents to the contrary notwithstanding, to lay emphasis on the fact that Freud from the first warned that repressed sexuality may take us to the lowest and perhaps dirtiest pathway to reality. For him the emphasis lay throughout upon the psychical factors of sexual life. Upon the capacity of the nearest lying sexual goal to exchange places with social values he based the possibility of a moral and higher culture. In these thoughts which form part of the support of the whole structure, I can in no way find anything that engenders scientific or ethical offense. I fully acquiesce. Yet there are, to be sure, in Freud's theses on sexual life not a few things which have rightly encountered sharp opposition. But into that I cannot here enter further than to give a cursory glance. Only one central thesis of Freud's need be referred to. The kernel of every neurosis was considered by the founder of psychoanalysis to be the repression of an incestuous desire which is directed towards the opposite-sexed parent and the jealous hate towards the like-sexed one. Every neurotic is a hindered Oedipus, he is sick because he may not espouse his mother or kill his father; every female neurotic must be considered an Electra in fantasy. That such impulses are to be met with in many neurotics must be admitted. But I am far from finding them in all. Hatred towards the father is not necessarily based on incest; it is even found in persons who have grown up motherless. But incest with the mother need not be a renewed child-wish and really often has only the sense of a regenerated fantasy: the dreamer himself might



develop it just to become another person or to form the life of another. While for Freud sexuality and love are very distinct, for Alfred Adler they mean little or nothing. It is to a high degree remarkable that one who knows how to observe so sharply and so finely certain lines of human life, especially the infantile, is in regard to the significance of sexuality and love really stricken with blindness. Adler derived all nervous phenomena and not a few character malformations from feelings of inferiority which revert to organic defects or insufficiencies. Sickness and the forms into which life is shaped follow therefore the fictive conductance lines (*fiktive Leitlinie*) to protest manfully against that inferiority, to work against the assurances (*Sicherungen*). Even the sexual fantasies serve only this fictive endeavor, which shows life in a false light. From the same source Adler derived along with neurosis, supersensitiveness, egoism, estrangement from reality, longing for power, malignancy, pride, immolating goodness, coquettish manners, cowardliness, and many other traits of character. His scholars continued the work: Fortmüller would deduce from the manly protest the whole of ethics, Kaus mendacity, Asnaourow cruelty, Wexberg anxiety, Lint in an especial case thievish inclination. So for Adler and his followers everything neurotic and the largest part of character malformations is displaced egoism; for Freud, so far as the Œdipus wish predominates, primarily unrequited love. Thereby Freud shows himself less one-sided than Adler, the former taking the ego impulse more earnestly than the latter did sexuality and love. Only Freud believed that checking the ego impulse primarily operates devastatingly when erotic reinforcements follow. To me it seems that the ego and love impulses *in concerto* are generally not to be separated from each other; every disturbance of the ego impulse influences very strongly the adjustment of love, and vice versa. Strong love precludes feelings of unreality as powerful pride forbids the highest love impulse. Only when one goes with or strengthens the other do the disturbances mentioned by Freud and Adler arise. Erotic failures affect self-feeling, reduction of self-feeling the courage of love. The impulse to repression and its after-effects can emanate from an encroachment upon the claims of the one or the other.

While according to Freud and Adler primitive impulse emotions dominate the realm below the threshold, Jung held that even highly important cultural and ethical impulsions operate in the subconscious. For Jung neurosis emanates from the inertia that establishes itself at certain development stages

and the resultant accommodation to reality, the inwardly bidden accommodation to higher cultural achievements itself resists and indeed reverts to an earlier childish relation. Inertia and indolence dispose the natural man to shrink from obvious duty; the actual conflict between inclination and duty drives him into neurosis, thereby into infantile customs, nay, into archaic functions. The dreamer, the neurotic, and especially the one suffering from dementia praecox, is atavistic. If as a result there appear sexual or incestuous fantasies, then they are (as Adler holds) only symbolically intended. In dreams, however, there is already concealed an attempt to interpret the inwardly prompted demands; in other words, to prepare an adaptation, which is by no means to say that this attempt takes the right course. Often the dream-effort at adjustment is intended only ironically. The task in any case consists in overcoming the inertia which opposes itself to the higher plane of development. Jung's hypothesis very strongly reminds one of the verses of Grillparzer:

"But children grow apace with years,  
And every stage betrays itself  
With restlessness and crankiness.  
Indeed, oft-times an illness shows  
We are the same and yet another;  
And in one mould are both lives cast.  
And this strange law within us rules:  
Our soul expands and round itself  
A wider circle circumscribes.  
And such illness all have endured. . . ."  
—*Judin von Toledo.*

In 1912 Freud in more restricted terms delineated the process of falling ill previously described by Jung (*Centralblatt f. Psychia., II. Jahrb., S. 298 ff.*). I must urge against Jung the objection that (1) by no means do all neuroses spring from inhibition to adaptation, and (2) indolence and inertia are an inadequate explanation of the origin of the resistance to the moral demands. As an illustration of how a neurotic development can appear, let us relate: A sixteen-year-old girl falls ill with violent headache and because of it is removed from school by the physician. As no improvement takes place, she makes complaint to me of her condition and adds that she is tormented in sleepless nights by the fear that she will become insane. The confession is introduced through the remark made in tears: "I suffer because there is no love among men!" It is easy to discover a very strong sentiment towards the brother. All other youths are dumbheads and coxcombs. Dreams disclose incest fantasies which surely are not merely

symbolically intended. Every thought of love and matrimony awakens aversion. The brother on the other hand desires that the sister address him before strangers with "Sie," and is furiously jealous of her. He suffers from suicidal impulses. It was easy to remove the inhibition and the headache as also the imperative ideas. It may also be added that the actual conflict is to be regarded as the next and most important cause of the repression in many cases. Freud has emphasized this as strongly as did Jung. It may be questionable to hold as Jung did that the source of neurosis lies in the tendency to resist adjustment to reality. If the thigh of a workingman is crushed by a falling block of stone one cannot well say that the imperfect adjustment of the bone is the cause of the fracture. It is condition, not cause. So when neurosis follows the death of loved persons I consider the incapacity to yield to the situation and to draw its consequences one of the conditions which made it possible for the repression to develop. Even there the early childhood is of importance so far as it can jointly determine the general features of the disease and at any rate revive again childish fantasies.

What has been said shows that one may not represent indolence or idleness as the real cause, the "*primum movens*," of repression and expulsion and the resultant defects of character before mentioned. But there is more to be added: Repressions that spring out of childhood may place an otherwise energetic man, one conscientious to the demands of duty, in a place which to another would be unimportant, viz., before an insurmountable barrier, so that the otherwise vigorous man plays a pitiable rôle. On investigation of such striking renunciation there will always be found associations which revert to childhood. Even healthy, yes all healthy people, have their idiosyncrasies which lead and force them to see and treat facts in a distorted way, and more particularly when they simulate repressed occurrences of earlier times. Even Jung admits that many neurotics are sick from youth, with whom the cause of the derangement is therefore not to be sought in an actual conflict (*Jahrb. V*, 354). If one bears this in mind he will see that, disregarding the rôle which Jung attributed to indolence (*Jahrb. V*, 422, 439), the difference between Freud and Jung is not great. It seems to me there are two groups to distinguish which are of prime importance in the study of youth: a *retention type*, with which the association comes from the past so that even a present or previous bagatelle assumes the proportions of a threatening monster, and a *repulsion type*, which in consequence of the trying present and

gloomy future throws even a normally developed man back into infantile fantasies and their automatic, possibly morbid, realization. The retention type makes out of an unpretentious present case of necessity or task a horrible affair, out of a gnat a camel. He constructs under the ban of the past a utopian present and future. Conversely, the repulsion type frightened back from the present treats the fantasies, which everyone has had and in the course of development quietly laid down, as something of prime importance, to live in and for which is the supreme task. Both types counterfeit reality and the attitude of the man to it: the retention type sees the past into the future, the repulsion type the present into the past,—both would press the present and the future into the mould of the past. The man who is *suspended between the two* projects the repressed events of the emotions belonging to the earlier time into some kind of temporal environment; the man *thrown back*, on the contrary, projects the emotions belonging to the present into the past, so that events of infantile and juvenile period, in themselves insignificant, become important and govern the point of view towards the present. The result in both cases is the same, the man becomes directed by the dreams of the past rather than by reality; that is not saying that in the aggregate personality must suffer a depreciation (cf. poets, artists, etc.). Retention and repulsion often operate conjointly.

The present differences between the various psychoanalytical tendencies do not indicate antagonistic principles, but rather important complements which in their totality constitute a healthy progress for the new movement which is so important for child study.<sup>3</sup> The errors will be corrected, the excesses smoothed off, but the method established by Freud and promoted by his students advances, to the welfare of science and art, to free men from fateful inward associations. I could herein only touch upon a few of the questions drawn from the huge domain of psychoanalytical activity, and, unfortunately,

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<sup>3</sup> Even Jung's "prospective tendencies" do not deviate in principle from Freud's prophetic dreams (*ankündigen Träumen*) except that Jung extended further than did Freud the function of the tentative anticipation of the future which is to be realized in the dream. In my "*Untersuchen über die religiöse Zungenrede*" (Jahrb. f. Psychoanal. Forschungen, Bd. III., 1 u. 2 Hälfte, also published separately by Deuticke, Vienna in 1911), I showed how in particular automatisms there is a reaching back to the past but at the same time with their help there is an attempt to attain a new future. Flournoy was the first to refer to the teleological significance of hallucination ("*Automatisme téléologique antisuicide*," Arch. de Psychologie, t. 7, p. 113-137, 1908).

could do this only in dry generalities unaccompanied by the interesting pictures of concrete experience. I must even refrain from stating the numerous laws founded upon psychoanalysis or even the fundamentally important facts of the opposition to analysis and the transfer of consciously formed contents and feelings to the analyst.

A rich banquet awaits those who value truth above tradition and who will not sleep when the swords clash in the battle for a great good cause. To be sure, even snares, manipulated by incredible means, await those who set foot upon newly discovered lands. But whoever takes up the fight cannot but accord with the beautiful sentiment of Romain Rolland's words: "The soul that once has looked upon the Countenance of Truth and seeks to deny it, destroys himself." That significant discoveries have already been achieved is not to be denied by even the embittered opponent of psychoanalysis. Much, yes, the most, is yet to be clarified. Whoever has the inclination and talents for earnest, arduous research work is invited to collaborate.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER and H. P. WELD

XX. ON THE LOCALIZATION OF PURE WARMTH SENSATIONS

By F. L. DIMMICK

In the experiments which this article reports we have studied the localization of points of warmth upon the cutaneous surface. The problem had already been attacked by Rauber<sup>1</sup> and by Ponzo,<sup>2</sup> but the results were not so definite as those obtained by Henri<sup>3</sup> and others in investigations of the localization of pressure. Rauber employed as stimulus the radiant heat derived from a heated metal object inserted in the holes of a small piece of wood laid upon the skin; his results, which will be noted later, were too meager to allow us to judge of the success of the method; but it would appear *a priori* that the pressure aroused by the wood must have served as a distraction. Ponzo used as stimulus a small drop of warm water, which fell from a pipette through which had been drawn a fine cotton thread knotted at the lower end. Although this stimulus admittedly aroused pressure as well as warmth, the experiments were continued in the belief that the observers were able, by direction of attention, to abstract from the pressure sensation. But there is no certainty that this was the case; and it therefore seemed advisable to repeat the experiments with a stimulus that should completely avoid the arousal of pressure.

*Method*

We first tried, in a long series of preliminary experiments, the method advocated by Toulouse,<sup>4</sup> and later employed by Kiesow and Ponzo<sup>5</sup> and by Piéron<sup>6</sup> in reaction experiments; but we found it

<sup>1</sup> A. Rauber. Ueber den Wärmeortssinn, *Centralbl. f. d. med. Wiss.*, 1869, no. 24, 372. We have not seen this article. Its method and conclusions are reported by A. Goldscheider, *Ges. Abhand.*, i, 179; T. Thunberg, in Nagel's *Handbuch der Physiol. des Menschen*, iii, 1905, 727; and E. Hering, in Hermann's *Handbuch der Physiol.*, iii, 2, 1880, 438.

<sup>2</sup> M. Ponzo, Studio della localizzazione delle sensazioni termiche di caldo e di freddo, *Riv. di Psic.*, ix, 1913, 393.

<sup>3</sup> V. Henri, Ueber die Raumwahrnehmungen des Tastsinnes, 1898, 90-141.

<sup>4</sup> E. Toulouse, N. Vaschide, et H. Piéron, *Technique de psychologie expérimentale*, 2d ed., i, 1911, 38.

<sup>5</sup> F. Kiesow u. M. Ponzo, Beobachtungen über die Reaktionszeiten der Temperaturempfindungen, *Arch. f. d. ges. Psych.*, xvi, 1910, 376.

<sup>6</sup> H. Piéron, Recherches sur les lois de variation des temps de latence sensorielle en fonction des intensités excitatrices, *Année psychologique*, xx, 1890, 34.

impracticable. Our observers, who had had extended practice in the analysis of cutaneous sensations, reported pressure in a large number of cases.<sup>7</sup> We, therefore, abandoned the method and turned to radiant heat. This means of stimulation which, as we have seen, was employed by Rauber, appears also to have been used by Tanzi in reaction experiments; he describes his stimulus as the "irradiation from the tip of a flame."<sup>8</sup> We resorted to an electrically-heated loop of platinum wire, which was enclosed in a tube in order to lessen the lateral radiation. The apparatus was made by running a piece of no. 20 copper wire through a glass tube 2 mm. in diameter, and another piece along the outside, and then connecting the ends by means of a loop of no. 24 platinum wire 1 cm. in length. This device was enclosed in a second glass tube, 4 mm. in diameter, in such a way that the loop came within 1 mm. of the end of the larger tube. The regular alternating current of the university lighting system was used in shunt with a Nichols' frame. An intensity of 8 to 10 volts was sufficient to bring the loop to a red glow in 1 to 2 sec.

A plaster cast was made of the arm of each observer.<sup>9</sup> The observer, seated at a table divided by a screen, placed his right arm, volar side up, in the cast which lay to the right of the screen. An area 18 by 8 cm. was marked off into 2 mm. squares by means of a rubber stamp. Tattoo marks were made on the median line, to insure the setting of the stamp in the same place at every trial. In preliminary experiments, a number of warm spots were found by

<sup>7</sup> These experiments were performed during the years 1912-13 by Mr. F. S. Kleinman under the direction of Dr. E. G. Boring. Many different sizes of pipettes were employed, but it was found impossible to obtain drops of water so small that they were not felt as pressure, even when the height of fall was not more than 2 mm. Later, the drop was applied by bringing it into contact with the skin, to which it adhered when the pipette was carefully removed. Still later, a tuft of cotton wool was substituted for the pipette. In this case, the drop was taken from boiling water and applied as quickly as possible. Again, however, about two-thirds of the applications aroused pressure as well as warmth. A fourth modification was tried: the tuft of wool and boiling water were employed as before, but now, in addition, a thin rubber membrane was stretched over the horizontal forearm; the pressure of the membrane was kept constant by hanging three weights (in all, 2 kg.) from the two edges of the membrane below the arm. The membrane was so thin that warmth was readily felt through it, and at the same time the skin was rendered less sensitive to pressure, either by adaptation or by a heightening of the limen. Even by this method, however, pressure was not entirely eliminated; it frequently happened that in a given sitting only a few pure warmths were obtained. Mr. Kleinman also attempted a modification of Rauber's method of obtaining radiant heat; the whole arm, except the region above a warm spot, was covered with a celluloid shield, and a very hot brass cylinder was brought close to the opening in the shield. The radiation, however, was not sufficient under these conditions to arouse warmth with any great regularity.

<sup>8</sup> E. Tanzi, *Revista sperimentale di freniatria e di medicina legale*, xvi, 1890, 396.

<sup>9</sup> We thought, at first, that casts would be necessary for the identification of the points in successive trials. The use of the rubber stamp made them unnecessary; but they were still a great convenience.

means of a von Frey warm-point. Those spots which gave a warmth of good intensity, and which were well removed from other warm spots, were chosen for the experiments. The area under experimentation was shaved, to prevent the arousal of pressure by touch of a hair.

The observer was required to indicate the point of stimulation on a life-sized photograph of the arm with the stamped area marked upon it. At no time during a series was he allowed to look at his arm.

The procedure was as follows. The observer was seated comfortably with his arm in the cast, and with his eyes either closed or directed on the photograph, as he preferred. The experimenter held the end of the stimulator, within two mm. of the skin, above the spot to be stimulated; gave the Ready signal; and closed the circuit. As soon as the observer felt a distinct warmth (in one to five seconds), he said 'There,' and the experimenter immediately broke the circuit. This was done in order that the stimulation might not become too intense. The observer now localized on the photograph the spot on his arm that had been stimulated. The actual process of localization differed with different observers, and will be described later.

Three observers took part in the experiments: Dr. E. G. Boring (*B*), instructor in psychology; Mr. G. J. Rich (*R*), an undergraduate in the department; and *D*, the experimenter. *B* and *R* observed in the systematic experimentation; after the regular work was completed, *R* served as experimenter with *D* as observer. This additional work was rough, and is taken into account only generally. Its main purpose was to give *D* an insight into the problem of localization from the observer's standpoint.

For the work with *B* and *R*, fifteen points upon or near the marked area were used, and ten series were worked through, making in all one hundred and fifty stimulations for each observer. With *D* only ten points were used, and only five series were taken. All of the fifteen points were stimulated in haphazard order in every series, and a rest of at least five minutes was given between series.

Besides the series with the warm stimulus, five series with a pressure stimulus were taken. A von Frey hair-aesthesiometer was employed as stimulator. The results from these series were employed for purposes of comparison and control.

### Results

*Method of Localization.*—Henri, in his investigation of pressure localization, finds that observers fall into two groups. The one group localizes only after deliberation and verification of the point, the other localizes the point almost immediately. Henri further distinguishes the members of the two groups as those with good and those with poor visual imagery. The first difference appeared, by chance, in our two observers; the second distinction, however, did not appear. Both *B* and *R* reported in the early experiments that localization seemed to be visual; but later they declared that it was largely kinaesthetic. The difficulties and methods of localization are noted in the introspective reports. *B* remarked, at the beginning of the experiments: "Crosswise localization was almost immediate and always first. Refer to one side or the other before looking at the photograph. Considerable difficulty as to place up and down; also transversely as to how near the edge the localization occurs, particularly in the region



C-R [see Fig. 1]. Photograph not real like the arm; lower and upper portions more real. In central part the localization is hard."

R's reports at first are not full; yet they give some indication of his difficulties. "I have a feeling of uncertainty. There are no objective criteria. Localization seems to depend upon the part of the arm attended to."

At the end of the second series B reported: "I get a visual image of the place on the arm, and place the point in relation first to the wrist and then to the elbow." Later on, however, he decided that the "localization was rather kinaesthetic than visual."

The same thing was noted in the introspection of R. "Visual image of arm and place where warmth is felt. Not very definite. Use after-images." Later he reported that he had a kinaesthetic 'feel' in the left arm when he had moved the pointer to the right place on the photograph.

D, in the rough work performed by him, localized visually. He found no kinaesthesia; but this negative result may be due to the fact that his experience was only one-third as great as that of the others. He confirmed the reports of B and R as to the uncertainty and difficulty of localization.

*Qualitative Observations.*—Several observations of a qualitative kind appear in the reports, particularly in those of B. It is to be remembered that at no time during the experiment was anything allowed to come into contact with the surface of the skin under experimentation. Yet both observers often reported 'pressure.' They were asked to attempt an analysis, in the hope that some explanation might be found. R found nothing; but B, who is unusually practised in cutaneous work, reported for one stimulation as follows. "Seemed to get a pressure. It has a dull, blurry, diffuse, smooth, almost soft, mushy character; and I should say that these terms were not merely attempts to describe the indescribable, but mean actual pressure." Again he reported: "Many of the warmths seem pressury. Once I got a sort of stingy, puckery feel, which (it occurred to me) might be what makes the warmth seem pressury. There seems to be a stage when there is a very faint sensation in which I am certain neither of the location nor of the quality. Later I come to place it and then to judge its quality. On the contrary, I did get one or two warmths definite as to quality, but very indefinite as to localization until they became stronger."

It is possible that the radiant heat causes the skin to contract, and thus produces a direct stimulation of the organs of pressure. Since, however, the results do not show that the observer is more accurate in his localizations when pressure is reported, we are forced to leave the question open.

*Results of Localization.*—Three general statements may be made: (1) there are very noticeable tendencies in direction of localization; (2) certain areas are preferred for localization; and (3) the error is very large.

(1) Let us first consider the tendencies of direction with regard to imaginary  $xy$  axes; the  $y$  axis running up and down through the centre of the mapped area, and the  $x$  axis crossing at right angles in the centre. If we examine the results of B, we find that the localization of points at some distance above the  $x$  axis tends toward P and the  $y$  axis; while that of points below the  $x$  axis tends toward C and (with one exception) toward the  $y$  axis. But points near the

$x$  axis, on both sides, may be localized *either up or down*. It follows that our ' $x$  axis' should be not a line but a band. Moreover, since there are two points, the one just on the  $C$  side and the other on the  $R$  end of the  $x$  axis, both of which have a tendency toward  $P$ , the band should also be somewhat turned (see Fig. 1). We may say in general, then, that the tendency is *in from the edges of the mapped area*.

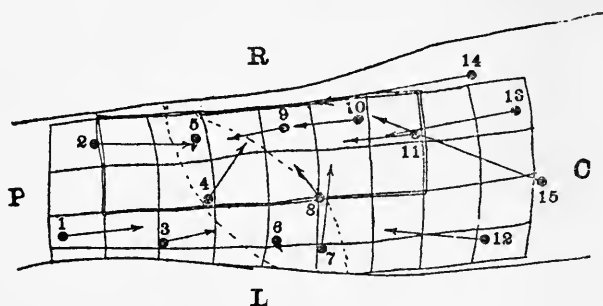


FIG. 1. Obs. B

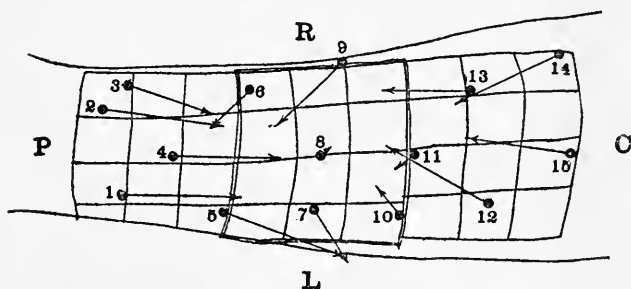


FIG. 2. Obs. R

The arrows in the diagrams indicate for any given point the average displacement of the localization both in amount and in direction. The values were found by computing the average component in the left-right direction and in the central-peripheral direction (adding algebraically for localizations made on different sides of the point), and constructing the hypotenuse upon these two components. The arrow-tip thus lies at the centre of gravity of all the localizations made. The length of the line does not represent the average error (Table III), which is computed independently of the direction of reference, but the average displacement in the direction indicated.

There is a striking resemblance between the results of  $B$  and those of  $R$ . If we turn to  $R$ 's results, we see that the  $y$  axis coincides with that of  $B$ . The  $x$  axis is even more regular, including an area 6 cm. wide, across the middle of the mapped area, at right angles to the  $x$  axis (see Fig. 2). Here, again, the tendency is *in from the edges*.

An attempt has been made in the following Tables to give a quantitative presentation of the tendencies. Table I shows the average or resultant tendencies in the *PC* and *RL* directions (see Figg.). Both the direction of error and its amount in that direction are taken into account. If, for instance, the given localization be considered as a force, then the *P* or *C* value represents the component force in the longitudinal direction, and the *R* or *L* value the component force in the lateral direction. The resultant from these two would give the amount and direction of the point's movement.

TABLE I

THE AVERAGE OR RESULTANT TENDENCIES (IN MM.) IN THE LONGITUDINAL AND LATERAL DIRECTIONS. DOWN (*P*), UP (*C*), RIGHT (*R*), LEFT (*L*)

No. of point	Observer B				Observer R			
	Directions				Directions			
	P	C	R	L	P	C	R	L
1 .....	....	30.5	1.9	....	....	48.2	....	4.2
2 .....	....	38.9	....	2.9	....	46.7	....	8.7
3 .....	....	20.2	2.5	....	....	33.6	....	13.3
4 .....	....	17.2	20.7	....	....	43.2	....	3.5
5 .....	0.9	....	....	3.1	....	48.8	....	13.0
6 .....	....	0.7	....	4.7	17.0	....	....	14.7
7 .....	....	6.2	33.6	....	....	12.2	....	22.4
8 .....	8.7	....	13.9	....	....	3.5	2.4	....
9 .....	28.2	....	....	2.3	28.8	....	....	28.3
10 .....	25.8	....	....	0.4	8.5	....	11.9	....
11 .....	28.5	....	0.7	....	8.4	....	....	5.5
12 .....	43.6	....	6.4	....	39.2	....	24.6	....
13 .....	52.9	....	....	3.4	35.0	....	1.4	....
14 .....	61.4	....	....	5.4	43.9	....	....	16.7
15 .....	66.0	....	32.7	....	40.8	....	7.8	....

In numbering the points, we begin at the *P* end of the area and go toward the *C* end. Consequently, the values in the *P* column of Table I begin at 0 and increase, while those in the *C* column begin high and decrease, as we go from *P* to the *C* end of the area. Those in the middle area have very small tendency-values as compared with the others.

The same thing for the lateral direction is shown in the *R* and *L* values. It does not appear so definitely, however, because the points are first on the one side and then on the other of the median line.

Since in Table I one large error in one direction may have more influence in the average than two or three smaller errors in the other direction, some of the averages do not fully evaluate the tendencies. We therefore present another Table, which exhibits the tendencies without regard to the amount of error. Table II indicates the tendencies of direction according to the actual number of localizations in every one of the four directions for every point. When either of the sums *P+C* or *R+L* is less than 10, the remaining localizations

were in a line with the point stimulated, and at right angles to the absolute direction in question; thus, if  $P+C=9$ , one point was localized on the line passing through the point and perpendicular to the median line. Table II, then, shows the same results as Table I, but in a different way. It also shows that the change in tendencies, as we go from the one end to the other, occurs not abruptly but gradually.

(2) The next point to be noted is the favoring of certain areas. These are outlined doubly in the Figg. For *B* the area lies along the *R* side of the mapped area, and contains  $66\frac{2}{3}\%$  of all localizations.

TABLE II  
THE TENDENCIES OF DIRECTION ACCORDING TO THE ACTUAL  
NUMBER OF LOCALIZATIONS IN EACH ONE OF THE FOUR  
DIRECTIONS FOR EVERY POINT

No. of point	Observer B				Observer R			
	Directions				Directions			
	P	C	R	L	P	C	R	L
1 .....	0	10	5	5	0	10	2	8
2 .....	1	9	2	7	0	10	2	5
3 .....	2	8	6	3	1	8	1	6
4 .....	4	5	10	0	0	9	2	3
5 .....	4	5	1	7	2	8	6	0
6 .....	5	5	5	6	3	7	2	8
7 .....	5	5	9	1	3	7	10	0
8 .....	6	4	9	1	6	4	7	3
9 .....	8	1	2	6	6	2	0	10
10 .....	8	1	5	3	7	3	9	1
11 .....	8	1	3	6	6	4	4	6
12 .....	9	1	7	3	7	3	7	2
13 .....	9	1	5	3	8	2	3	3
14 .....	10	0	2	6	10	0	2	7
15 .....	10	0	9	1	10	0	5	5

An illustration may assist in making the table clear. Spot No. 9 (Obs. B) was localized 8 times below the point, once above the point, and once neither above nor below. The same spot was also localized twice to the right, 6 times to the left, and twice neither to the right nor to the left.

For *R* the area is coincident with *x*, *i. e.*, it is 6 cm. wide, and extends across the middle (up and down) of the mapped area. It contains 50% of all the localizations. *B* reported that he was "prejudiced by the stamped area, in that there was a tendency to localize well within it." This prejudice does not, however, account for the extreme favoring of a single area, a fact for which there is no explanation in the observers' reports.

(3) Finally, the error of localization is itself exceedingly large. Of the 300 localizations made by the two observers, only 4 were correct, *i. e.*, fell within the area of 4 mm. A larger number might have been expected to occur by chance. Only 20 localizations were made

with an error of less than 10 mm. The extremes were 146 mm. for *B*, and 132 mm. for *R*. Table III gives the average errors for each point, and their mean variation.

We have here a rough measure of the inability of the observers accurately to localize pure warmth. We find average errors ranging from 15 to 63 mm., and mean variations often more than half the value of the average error.

*Pressure.*—It was said above that five series were performed with a pressure stimulus for the sake of comparison with the main series. The results obtained with pressure were similar to those got by Henri under similar conditions and by the same method. The errors were comparatively small, the average error for *B* being about

TABLE III

AVERAGE ERROR (IN MM.) AND MEAN VARIATION FOR EACH POINT

No. of point	Observer B		Observer R	
	A.E.	M.V.	A.E.	M.V.
1 .....	30.9	15.28	57.6	25.12
2 .....	28.1	14.80	50.4	31.80
3 .....	27.9	9.70	43.4	20.04
4 .....	35.2	14.58	43.2	21.80
5 .....	15.8	8.96	58.8	27.60
6 .....	24.6	15.60	33.5	16.10
7 .....	44.8	12.60	31.2	11.90
8 .....	22.2	5.04	25.0	15.20
9 .....	31.1	11.48	46.6	18.50
10 .....	24.4	16.00	25.3	12.22
11 .....	32.1	18.12	22.2	6.60
12 .....	52.5	22.40	57.9	25.12
13 .....	51.6	9.58	51.1	16.68
14 .....	59.9	25.00	46.4	17.78
15 .....	63.8	25.04	45.1	12.10

11 mm., and for *R* about 20 mm.,<sup>10</sup> and the localizations were clustered around the point of stimulation instead of tending in a single direction as was the case with warmth. It seems, then, that the observer has far less ability to determine the position of a point on the skin when it is stimulated by warmth than when it is stimulated by pressure.

*Previous Work.*—A comparison of these results with those of previous investigators of the localization of warmth shows both agreement and disagreement. Rauber concluded that the 'warm circles' are larger than the corresponding 'touch circles.' In so far as this statement means that the error of localization of warmth is greater than that of the localization of pressure, we agree. Ponzo reached the same general conclusion, although his average errors and mean variations are much smaller than ours. Furthermore, Ponzo's results show

<sup>10</sup> These average errors are larger than they should be, because only five series were taken, and a few large accidental errors raised the average.

errors of direction which group about the point of stimulation, while the results of our experiments show a tendency to localize certain points in certain areas. These differences are doubtless attributable to differences in method. Ponzo employed a stimulus that was not free from pressure, his observers localized by touching the arm, and the warm spots were localized but five times as against our ten.

## XXI. FORM *vs.* INTENSITY AS A DETERMINANT OF ATTENTION

By L. G. MEADS

President Sanford once asked the senior editor of these studies if it might not be possible to compare form and intensity of stimulus, in some quantitative way, as regards their power to compel the attention. The present experiments give a first, rough-and-ready answer to this question in a single case. A light-form of varying intensity was compared with a 'formless' spot of light of fixed intensity, and that ratio of the objective intensities was determined at which the two stimuli were judged equally clear.

*Apparatus.*—The apparatus consisted of a projection lantern with tachistoscopic attachment; a black box with three openings, one to admit the light of the lantern, and two front circular openings (8 cm. in diameter, 18 cm. apart) covered with ground glass, through which the light passed to a large ground glass screen (150 by 105 cm.); and an episcotister placed between box and screen. The tachistoscope gave an exposure of a tenth of a second ( $110 \pm 8$  sigma). One-half of the screen was covered with a sheet of thick paper, in the centre of which was cut a square cross (9 by 9 cm.; width of arms, 4 cm.); the episcotister was placed always on the same side as the cross. A small electric bulb, set midway between the stimuli, served as fixation-point. The 'formless' spot was, of course, roughly circular; it had a bright central area of some 12 cm. in diameter, and a total diameter of some 17 cm., and was therefore considerably larger than the cross; it made an impression of vagueness and diffuseness, and the observers never named its form, but referred to it as the 'spot' or 'patch' of light.

The observations were made in the dark room and with dark adaptation. The observers sat at a distance of 3.75 m. from the screen.

*Method.*—The instructions were as follows: "At the signal Now! you will fixate the spot on the glass; a second and a half later, two spots of light will be shown. You are to report which of the two (if either) is the more clear." The observer was further instructed that observations would be made in series, in some of which the one and in others the other spot would at first be definitely the clearer.

In the descending series the light of the cross was cut down to  $180^\circ$  of the episcotister, while the formless spot was presented at full intensity; in the ascending series the light of the cross was cut down to  $84^\circ$ . Preliminary experiments had shown that at  $180^\circ$  the cross would always be the clearer, and at  $84^\circ$  would always be the less clear stimulus. The steps in both series were  $12^\circ$ .

Two observers made 40 series; ten ascending and ten descending with the cross on the right, and the same number with the cross on

the left. These observers were Professor H. P. Weld, and Mr. J. S. Smith, a graduate student. Three observers made 8 series; two ascending and two descending with the cross on the right, and the same number with the cross on the left. These observers were Mr. H. G. Bishop, assistant in psychology, and Drs. E. G. Boring and W. S. Foster, instructors.

Judgments were passed in the form 'spot' (or 'patch'), 'cross,' 'equal.' Preliminary experiments showed both that there is a tendency to name the clearer stimulus, and also that, if the judgments 'clearer,' 'less clear' are required, the attention is likely to be focussed upon the one or the other stimulus with a view to comparison.

*Results.*—With full intensity of the formless spot, judgments of equal clearness were obtained for the following intensities of the cross:

Observer	Degrees	M. V.
H. P. W.	135	9.2
J. S. S.	136.5	13.7
H. G. B.	132	21.0
E. G. B.	118.5	8.0
W. S. F.	142.5	16.8

The following excerpts from the introspective reports may be quoted:

E. G. B.—"I am conscious of a difference of intensity even when judging clearness in the reverse order." "The judgment of clearness seems very much tied up with intensity. I would sometimes think 'That is intense enough to make them equal in clearness,' although the intensities were obviously unequal." "I noticed the clear-cutness of the cross even though the spot was the more clear."

H. G. B.—"Easy to judge. Clearness stood out in one even though intensity was very much less in that one."

W. S. F.—"You seem to have to let yourself be blank, or rather to attend to both,—no previous attending to one. You attend to both, only one is clearer than the other."

H. P. W.—"The judgment is very easy provided only the observer can avoid any sort of expectancy before the stimulus appears. If, however, one should expect 'spot' (let us say) to be more clear, then one is very apt to judge it so, unless the difference in intensity is very great."

*Conclusion.*—We conclude that a light-form of relatively low intensity may have as great a power to attract attention as a formless light of relatively high intensity. In the concrete, a cross illuminated by some 133° of the light of a 25 w. 110 v. mazda lamp sent through two ground glasses may be as clear as a formless patch illuminated by the full 360°.

This is a single result, obtained with a somewhat rude technique; but the technique can be improved; and the ease of judgment renders it probable that the comparison of form and intensity can be made with other material.

## XXII. THE DETERMINATION OF THE LIMENS OF SINGLE AND DUAL IMPRESSION BY THE METHOD OF CONSTANT STIMULI

By E. J. GATES

In their discussions of the method of Constant Stimuli both Müller<sup>1</sup> and Titchener<sup>2</sup> employ a set of results which were obtained by Riecker in a determination of the two-point limen.<sup>3</sup> These results are far from satisfactory; but, although Titchener advocates the use of the method of Constant Stimuli for the determination of the two-point limen, nothing better has as yet appeared. It was our original purpose to repeat Riecker's experiment, not only in the hope of obtaining a more nearly regular set of results, but also as a further test of the method. Later, however, a modification of the experiment, by the admission of a larger number of categories than the usual one-point, two-point and doubtful judgments, seemed advisable. In preliminary experiments, our observers reported that, between the impression of a single point, and that of two discrete points, there occurred a number of impressions which proved to be similar to those which, since the experiments of Henri and Tawney,<sup>4</sup> Tawney,<sup>5</sup> and Judd,<sup>6</sup> are known as 'point,' 'circle,' 'line,' 'dumb-bell,' etc. The method of Right and Wrong Cases regarded these impressions as belonging to the category 'one-point'; but there seemed to be no reason why the judgments 'circle,' 'line,' etc., might not be accepted, and their frequencies treated as psychometric functions, precisely like the 'two-point' judgments. When our experimental work was completed, we submitted the data to Professor Urban, who assured us that, in his opinion, any number of categories might be accepted, and that the notion of psychometric functions was adequate to the treatment of our data.<sup>7</sup>

*The Experimental Procedure.* Since we set out to repeat Riecker's experiment, we worked on the lower eyelid of the right eye, and we used the same nine D's: 0, 0.5, 1, 1.5, 2, 3, 4, 5, and 6 Paris lines (1 Paris line = 2.27 mm.). We employed the Griesbach aesthesiometer, with a pressure of 10 grams on each point as indicated by the scale of the instrument. We added, however, points of hard rubber, whose weight brought the pressure up to 19.5 gr.; this pressure, as measured by a chemical balance, was fairly constant; and in any case the m. v. of the instrument was probably exceeded by irregularities of applica-

<sup>1</sup> G. E. Müller, *Gesichtspunkte u. Tatsachen d. psychophysischen Methodik*, 1904, 38ff.

<sup>2</sup> E. B. Titchener, *Experimental Psychology*, 1905, II, i, 92ff.; ii, 250.

<sup>3</sup> A. Riecker, *Versuche über den Raumsinn der Kopfhaut*, *Zeit. f. Biologie*, 1874, 177ff.

<sup>4</sup> V. Henri and G. Tawney, *Ueber die Trugwahrnehmung zweier Punkte bei der Berührung eines Punktes der Haut*, *Philos. Studien*, xi, 1895, 394ff.

<sup>5</sup> G. Tawney, *The Perception of Two Points not the Space-threshold*, *Psychological Review*, ii, 1895, 585ff.

<sup>6</sup> C. H. Judd, *Ueber Wahrnehmungen im Gebiete des Tastsinnes*, *Philos. Studien*, xii, 1896, 409ff.

<sup>7</sup> We are glad to acknowledge our indebtedness to Professor Urban not only for his criticism and advice, which have been most helpful, but also for his interest in our results.



tion, although very great care was taken to handle the aesthesiometer in the same way throughout the experiments. The added points were very slightly conical, and had a terminal diameter of 1.15 mm. The nine *D*'s were arranged by hazard into 100 series, so that every stimulus was presented 100 times, and 900 experiments were performed upon every observer. Those who took part in the experiment were Dr. E. G. Boring (B), then assistant in Psychology; Dr. C. A. Ruckmich (R), then instructor in Psychology; and Asst. Prof. H. P. Weld (W). The observers lay supine on a low couch, with eyes closed; the experimenter sat on a chair at the head of the couch, and, holding the instrument in both hands, gave the ready signal and applied the stimulus. The judgment was given as nearly immediately as might be; and the experimenter, holding the aesthesiometer in his left hand, entered the judgment by a symbol on the record-sheet, and then set the instrument for the next experiment. The whole procedure, from experiment to experiment, occupied 10 or 12 seconds. At the end of the series, however, a longer period (30 to 60 sec.) was allowed, in order to prevent adaptation; and on the resumption of the experiment the beginning of a new series was announced. A few weeks previously, the experimenter had put each one of the observers through a practice-series of 900 experiments, so that the work proceeded with almost machine-like regularity. The experiments were performed between 2.30 and 4 o'clock on three afternoons of the week; a single sitting lasted for about thirty minutes; and in that time about ten series were run off. After the preliminary work was finished, therefore, about four weeks were required for the collection of the data.

In the preliminary experiments the observers were limited to the three usual categories. When it was decided to accept other types of judgment, the observers themselves were allowed to choose their terms. The observations of W and of R are hereafter discussed under the headings 'point,' 'point or circle,' 'circle,' 'circle or line,' 'line,' 'line or dumb-bell,' 'dumb-bell,' 'dumb-bell or two-points,' and 'two-points.'<sup>8</sup> Observer B adopted only five categories: 'one-point,' 'one-point or extended,' 'extended,' 'extended or two-points,' and 'two-points.' For all observers, the 'or' judgments were regarded as doubtful judgments.

*The Results.* The results of such an experiment consist, of course, in the numbers expressing the relative frequencies with which the different *D*'s were judged under the different categories. These results are shown in Tables I and II. Before we proceed to their

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<sup>8</sup> These names were, in fact, used only by W. Introspectively, however, R's perceptions so closely resembled those of W. that for purposes of comparison the one set of names may be employed. The principal differences are that for W the third category included impressions which were circular, as if the stimulus was a large blunt point, while for R impressions in this category were slightly oval and nucleated; again, for W the 'line' had a definite width and was rounded at the ends, while R described it as a long narrow oval with two nuclei.

TABLE I  
OBSERVED FREQUENCIES OF JUDGMENTS OF THE NINE D's  
UNDER NINE CATEGORIES

Observer W									
D	point	point circle	circle	circle line	line	line d-bell	d-bell	d-bell two	two point
0	81	4	10	..	3	..	2	..	..
0.5	18	2	65	..	13	..	2	..	..
1	13	3	58	8	17	..	..	..	1
1.5	14	2	59	1	14	4	2	2	2
2	11	1	56	5	20	..	3	1	3
3	3	1	29	2	35	7	16	3	4
4	..	..	7	2	24	4	19	13	31
5	..	..	..	..	1	2	3	6	88
6	..	..	..	..	..	..	..	..	100

Observer R									
D	point	point circle	circle	circle line	line	line d-bell	d-bell	d-bell two	two point
0	53	12	27	1	5	..	..	1	1
0.5	10	7	50	6	17	..	8	..	2
1	4	5	52	8	18	3	8	..	2
1.5	4	11	46	9	16	2	7	..	5
2	9	9	39	4	22	1	10	1	5
3	7	2	22	4	17	1	28	2	17
4	1	..	4	..	6	4	39	1	45
5	..	..	3	..	2	..	15	2	78
6	..	..	..	..	..	2	6	3	89

TABLE II  
OBSERVED FREQUENCIES OF JUDGMENTS OF THE NINE D's  
UNDER FIVE CATEGORIES

Observer B					
D	point	point extended	extended	extended- two-point	two-point
0	89	1	9	..	1
0.5	63	2	33	1	1
1	56	6	36	1	1
1.5	57	3	36	1	3
2	51	2	41	2	4
3	28	4	50	5	13
4	5	2	37	16	40
5	1	..	5	5	89
6	..	..	..	2	98

mathematical treatment, we may call attention to certain facts which the Tables reveal. (1) The relative number of 'or' judgments for all observers is very small. Two hypotheses suggest themselves. There may have been an unconscious tendency to avoid doubtful judgments, due either to the positive tendency to judge under a specific category, or to the mere fact of a considerable number of possibilities of judgment. Or, again, the perceptual experiences may not have 'shaded off' one into another; the regions of particular judgments may have been too large, and the categories too flexible, to permit of other

judgments. This supposition seems to be borne out by another result of Table I. (2) The total number of 'circle' judgments is out of all proportion to the others. At first thought one might suspect a change in standard of judgment; but that is unlikely, since the proportion of these judgments to the total number of judgments is approximately the same for both observers (31% for W, and 29% for R). Apparently, then, either the region was large, or the category admitted a large number of different impressions. Finally, (3) it may be observed that while the curves of frequencies are far from ideal they are yet fairly regular. Observer B has a single inversion of 1%; W has one inversion of 4% and four of 3% or less; R has one of 6%, two of 5%, and five of 3% or less.

The method of treating results where more than three categories are admitted has been indicated by Urban in his examination of Keller's acoumetrical experiments.<sup>9</sup> In these experiments five kinds of judgments were allowed; and since, in our experiments, B also had five categories, the method of treatment in the two cases is so far identical. The procedure, in brief, is as follows. We first determine in the usual way the constants for the psychometric functions of the One-point judgments, and for those of the Two-point judgments. Their curves obviously give the boundary, on the one hand between the One-point and the Point-extended judgments, and on the other hand between the Two-point and the Two-point-extended judgments. If, now, we combine the frequencies of the One-point and of the

TABLE III

Boundary	Observer B					Interval
	h'	c'	h	c	L	
1-1X.....	-.1963	0.5350	-.3926	-0.6428	1.636	
1X-X....	.2220	-0.6211	.4440	0.7110	1.601	-0.035
X-X2....	.2781	0.4040	.5562	2.0726	3.724	2.123
X2-2.....	.2867	0.5639	.5734	2.2841	3.980	0.256
	Observer R					
	h'	c'	h	c	L	
p-pc.....	-.1642	1.3180	-.3284	0.3328	-1.012	
pc-c.....	-.1712	1.3900	-.3424	0.3628	-1.060	-0.048
c-cl.....	-.1989	0.4601	-.3978	-0.7333	1.843	2.903
cl-1.....	-.2117	0.3891	-.4234	-0.8811	2.081	0.238
1-ldb....	.2455	-0.0837	.4910	1.3893	2.830	0.749
ldb-db ..	.2440	-0.0379	.4880	1.4261	2.923	0.093
db-db2...	.2328	0.4713	.4656	1.8681	4.012	1.009
db2-2....	.2333	0.5225	.4666	1.9323	4.150	0.138
	Observer W					
	h'	c'	h	c	L	
p-pc.....	-.2826	1.5970	-.5652	-0.0986	0.174	
pc-c.....	-.2909	1.5880	.5818	-0.1574	0.270	0.096
c-cl.....	-.2570	0.4266	.5140	-1.1154	2.170	1.900
cl-1.....	.2396	-0.2553	.4792	1.1823	2.467	0.297
1-ldb....	.2682	0.2379	.5364	1.8471	3.438	0.971
ldb-db ..	.2735	0.3102	.5470	1.9512	3.567	0.129
db-db2...	.3154	0.6030	.6308	2.4950	3.960	0.393
db2-2....	.3316	0.7029	.6632	2.6927	4.060	0.100

<sup>9</sup> F. M. Urban, Ueber die Methode der mehrfachen Fälle, *Arch. f. d. ges. Psychologie*, xvii, 1910, 380-387.

Point-extended judgments, and determine their constants, we shall obtain the lower boundary of the Extended judgments; and again, if we combine the psychometric functions of the Two-point judgments and those of the Two-point-extended judgments, and determine their constants, we shall obtain the upper boundary of the Extended judgments.

The same process of calculation applies when more than five judgments are accepted. We successively combine the frequencies from the two extremes, until we arrive at three or two judgments. Where five categories are employed, as by B, we arrive at four boundaries; where there are nine kinds of judgment, as in the cases of R and W, there are eight boundaries, since the lowermost and the uppermost boundary in every case is infinity. The constants of the psychometric functions which give the curves of these boundaries are shown in columns 3 and 4 of Table III. The values of  $h'$  and  $c'$  are derived from the normal equations of the method of least squares. The quantities necessary for setting up these equations were obtained from Urban's tables.<sup>10</sup> The values of  $h$  and  $c$  are derived from  $h'$  and  $c'$ ;  $h = h'/d$ , where  $d = 0.5$ , and  $c = 3h + c'$ . The limens, which are derived from  $h$  and  $c$ ,  $L = c/h$ , are presented in the fifth column of the table. In the sixth column are to be found the intervals of uncertainty. The interval represents in any case the difference between the upper and the lower boundary; for us it represents the area of the region covered by a category.

An inspection of the table reveals inversions of the first two categories by B and R. B's inversions, however, are very slight, in the second decimal; those of R are negative. The first of these probably means that even one point is judged as 'point or circle' more often than 'point.' The sense of the second seems to be that the tendency to give 'point' judgments for the least separation (0.5 Paris line) is equal to (or slightly greater than) the tendency to judge 'point' at no separation. There is good agreement as regards the Two-point limen: the average for all three observers is  $4.06 \pm .08$ . Riecker's Two-point limen was 1.88, a value which for our observers is approximately the limen of the first lower boundary of the third category. Riecker, apparently, either ran foul of the stimulus error, or purposely included the intra-cutaneous impressions in his Two-point judgments. The intervals of uncertainty are what we should expect from an inspection of the table of frequencies. Although it is here that the experiment is seen at its worst, we cannot say whether the irregularities are due to some inherent irregularity in the perceptual patterns, or whether they result from a change in standard of judgment. The observers reported a difficulty in maintaining the same standard and regretted that they had not had extended practice-series with the new categories. It is remarkable, on the other hand, that the interval of 'circle' is the largest interval for both R and W; that the size of this interval closely resembles that of B's 'extended'; that for W the intervals of circle, line and dumb-bell become progressively smaller as the two-point limen is approached; and that a similar relation is found between W's 'or' judgments. Aside from the fact that R's intervals do not show these relations so regularly, they differ materially from those of W only in the size of the interval for 'dumb-bell.'

In conclusion we may say that, if we were repeating the experiment,

<sup>10</sup> F. M. Urban, Hilfstabellen f. d. Konstanzmethode, *Arch. f. d. ges. Psychol.*, xxiv, 1912, 236ff.

we should give our observers the benefit of greater practice in judgment under the various accepted categories. We believe that a better set of *D*'s could be found; and we are sure that the cutaneous area chosen is not the best possible. We should take introspections at the close of each day's work.

## APPENDIX

The following figures were obtained by a pair of students in the regular work of the laboratory course in Quantitative Psychology. The *E* was G. J. Rich, the *O* was F. L. Dimmick. The method of procedure, the *D*'s, the cutaneous area, etc., were the same as in our experiment. Five categories were admitted: point, circle, oval, long-oval, two-point. Fifty series were made. In the calculation, the five categories were reduced to three in the way described above, and the directions in Titchener's Manual were then followed. The quantities for the equations were taken from Urban's tables. The results are:

Boundary	<i>h</i> '	<i>c</i> '	<i>h</i>	<i>c</i>	<i>L</i>
pt-circle.....	.1839	—0.9092	.3678	0.1942	0.528
c-oval.....	.2158	—0.2471	.4316	1.0577	2.451
o-lo.....	.2353	—0.2903	.4706	1.1215	2.383
lo-2.....	.2913	1.2110	.5826	2.8688	4.924

The figures are a sample of the sort of results that may be obtained by conscientious work in a limited period of time (4 afternoons), by students who have had only the practice that the laboratory drill-course affords.

## BOOK NOTES

*Dynamic evolution; a study of the causes of evolution and degeneracy.*  
By CASPAR L. REDFIELD. New York, G. P. Putnam's Sons, 1914.  
210 p.

The author starts out with the rather startling proposition that scientists think they believe in evolution but really believe in special creation. They have made it probable that animals today are the genetic descendants of different animals of earlier ages, but their explanation of how existing animals took their present form involves nothing but special creation. Present animals are descended from uni-cellular organisms but human intelligence was not derived from man's ancestors because they did not have it, and so it must have been created. The chapters are on energy in general and in animals, horse-breeding, 2.10 trotters, the effects of two generations, English setters, Holstein-Frisians, man and longevity.

*Recherches sur les sociétés d'enfants.* Par J. VARENDONCK. (Institut Solvay, Notes et Mémoires, Fascicule 12). Bruxelles, Misch & Thron, 1914. 93 p.

This study is in some respects a model of its kind. It is one of the most comprehensive studies of children's societies and these are classified and the origin of the groups is described. A special chapter is devoted to the chief or leader, and the psychology of the members, the collective conscience, attitude toward strangers, are also treated.

*What men live by.* By RICHARD C. CABOT. Boston, Houghton Mifflin, 1914. 341 p.

These essays are of extremely different values but all are breezy and original and the whole thirty-four of them are divided into (Part 1) work, (Part 2) play, (Part 3) love, (Part 4) worship. The author's characterization of love is by far the best part of the book and is as sane and wholesome reading as we have seen for many a day and involves the highest conceptions of wedded life.

*Poetik.* Von RICHARD MÜLLER-FREIENFELS. Leipzig, B. G. Teubner, 1914. 98 p.

*Vorträge zur Einführung in die Psychologie.* Von HANS KLEINPETER. Leipzig, J. A. Barth, 1914. 435 p.

*Grundzüge der Psychotechnik.* Von HUGO MÜNSTERBERG. Leipzig, J. A. Barth, 1914. 767 p.

*An introduction to general psychology.* By ROBERT MORRIS OGDEN. New York, Longmans, Green, 1914. 270 p.

*Grundriss der Psychologie für Juristen.* Von OTTO LIPMANN. 2d ed. rev. and enl. Leipzig, J. A. Barth, 1914. 95 p.

- Outline of a study of the self.* By ROBERT M. YERKES and DANIEL W. LARUE. Revised from the edition printed for the authors in 1913. Cambridge, Harvard University Press, 1914.
- Behavior; an introduction to comparative psychology.* By JOHN B. WATSON. New York, Henry Holt & Co., 1914. 439 p.
- An experimental and introspective study of the human learning process in the maze.* By FLEMING ALLEN CLAY PERRIN. (Psychological Monographs, Vol. XVI, No. 4, July, 1914). Princeton, N. J., Psychological Review Co. 97 p.
- Psychological experiments.* By J. V. BREITWIESER. Colorado Springs, Apex Book Co., 1914. 187 p.
- Laboratory manual for human physiology; experimental studies in hygiene, sanitation and physiology.* By CARL HARTMAN. Yonkers-on-Hudson, World Book Co., 1914. 144 p.
- Is conscience an emotion? Three lectures on recent ethical theories.* By HASTINGS RASHDALL. Boston, Houghton Mifflin Co., 1914. 200 p.
- Contributions to the analysis of the sensations.* By ERNEST MACH. Translated by C. M. Williams. Chicago, Open Court Publishing Co., 1910. 208 p.
- Der Traum ein assoziativer Kurzschluss.* Von Dr. HANS HENNING. Wiesbaden, J. F. Bergmann, 1914. 66 p.
- Report from the Department of Pathology and the Department of Clinical Psychiatry, Central Indiana Hospital for the Insane.* 1911-1912 and 1912-1913. Vol. V. Indianapolis, Wm. B. Burford, 1914. 380 p.
- Plato: Ion, with introduction and notes.* By J. M. MACGREGOR. Cambridge, University Press, 1912. 45 p.
- What is paranoia?* By E. STANLEY ABBOTT. (Reprinted from American Journal of Insanity, Vol. LXXI, No. 1, July, 1914, p. 29-40.)
- Anatomy and physiology of the eye and its appendages.* By JOHN WELSH CROSKEY. Philadelphia, Smith-Edwards Co., 129 North 12th St. 18 p.
- Certain further factors in the physiology of euphoria.* By GEORGE V. N. DEARBORN. (Reprinted from the Psychological Review, Vol. XXI, No. 3, May, 1914, pp. 166-188.)
- Our sexual birthright.* By GEORGE V. N. DEARBORN. (Reprinted from Mind and Body, September 1914, Vol. 21, no. 231, p. 215-223.)
- The self and the ego.* By KNIGHT DUNLAP. (Reprinted from the Psychological Review, Vol. 21, No. 1, January, 1914, pp. 62-69.)
- Images and ideas.* By KNIGHT DUNLAP. (Reprinted from the Johns Hopkins University Circular, March, 1914.) Baltimore, Johns Hopkins Press, 1914. pp. 25-41.

### THEODOR LIPPS

The death is announced, at the age of sixty-three years, of Professor Lipps of Munich, well known to psychologists by his systematic work *Grundtatsachen des Seelenlebens* (1883; reprinted 1912) and by his contributions to the theory of space-perception and to psychological aesthetics. Lipps, who has been in retirement for some years on account of ill-health, was a keen and incisive thinker, and master of a remarkably trenchant style. He was by temperament logician rather than psychologist; but a strenuous logic is indispensable to psychological theory; and Lipps' criticisms were always of positive value, though his constructive work—ingenious and suggestive as it undoubtedly was—did not find general acceptance.

Lipps' other works are: *Psychologische Studien* (1885, 1905); *Der Streit über die Tragödie* (1890); *Aesthetische Faktoren der Rauman-schauung* (1891); *Grundzüge der Logik* (1893, 1912); *Zur Psychologie der Suggestion* (1897); *Raumaesthetik und geometrisch-optische Täuschungen* (1897); *Komik und Humor, eine psychologisch-aesthetische Untersuchung* (1898); *Ethische Grundfragen* (1889, 1912); *Leitfaden der Psychologie* (1903, 1909); *Selbstbewusstsein, Empfindung und Gefühl* (1901); *Vom Fühlen, Wollen und Denken* (1902, 1912); *Aesthetik* (1903-6); *Naturwissenschaft und Weltanschauung* (1906); *Philosophie und Wirklichkeit* (1907); *Naturphilosophie* (1907); *Psychologische Untersuchungen* (editor, 1907 ff.); *Beiträge zur Aesthetik* (co-editor, 1890 ff.).



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## AUTOMATIC WRITING

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### I. INTRODUCTION

The purpose of the investigation under consideration was, if possible, to obtain samples of automatic writing in order to determine (1) Under what conditions such writing appears, and (2) What changes take place in the appearance of writing as it becomes less and less controlled.

The first problem involved a study of the mental make-up of a given reagent relative to the scope of attention, tendency to automatism, nature of the inner speech, etc.

The second problem would include a comparison of the writing produced under distraction with the normal writing of a given reagent with reference to many different writing-factors such as size, alignment, slant, line-quality, form and the like. Such a comparison is highly desirable in connection with the study of handwriting for diagnostic purposes since it is of prime importance to know what changes appear as writing becomes progressively uncontrolled. Such a knowledge would also be of value in investigating the alleged shifts of handwriting individuality in mediumistic writing.

In order to obtain material for discussion of the above-stated problems the following experiments were tried:

I. Preliminary experiments, reagent took dictation and wrote sentences or answered questions while reading silently.

II. Reagent wrote a given verse, perfectly memorized, continuously, during the silent reading of the chapters of a novel.

III. Reagent wrote a given verse, perfectly memorized, during the continuous adding of columns of figures.

IV. The reagent wrote, continuously, a given verse, perfectly memorized, while reading aloud.

V. The reagent wrote from dictation (1) while reading aloud, (2) while reading silently.

In reporting our results we confine ourselves to their bearing upon the first problem stated.

Before a detailed account is attempted of the conditions holding in the different series of experiments, a general statement of the conditions that were constant throughout the whole may be given. The experiment ran continuously for seven months (with the breaks incident to college organization), occupying an afternoon session from one to two hours long for five days in the week. The two writers of the paper (A and D) served in turn as reagents.

The reagent in every case sat to the left of a black screen, in the lower half of which was a loose sleeve for the arm opening toward the right in such a fashion that when the hand and arm had been run through it, they were perfectly concealed from the reagent and rested comfortably and without constraint upon the writing-pad which lay on the table, to the end of which the screen was clamped. The experimenter sat to the right of the screen by the table. He was, therefore, out of the reagent's sight. His part in the experiment consisted in manipulating the writing-pad, timing by means of a stop-watch the writing of the reagent, and taking down at set intervals the introspections of the reagent. The writing was done, in part, in pencil, in part, in ink.

A few words as to the reagents. Neither shows any striking automatic tendencies, neither is absent-minded, both are averse to "letting themselves go" and maintain control to a high degree. Both show certain perseverative tendencies, are apt to be obsessed by their work, have difficulty in getting to sleep, etc.

Whatever automatisms appear for D would seem to be sensory in character. D has little control over her imaginal processes and is often greatly surprised by the course of her images, she is strikingly "intuitive" in certain lines of work. With this there goes a consciousness of movement to an extraordinary extent so that D is able to inhibit motor expression to an unusual degree. She is a "bad subject" for the muscle-reader. Such inhibition of effort and excess of control are evident in her handwriting which is small, labored, and self-conscious. Sudden impulses leading to excessive

movement occur, however, at intervals and under certain physical conditions D's writing becomes free, fluid, and large.

A is very easy and rapid in his movements, his writing is unusually rapid and smooth in impulse, although slightly illegible. He cuts strokes and ignores the visual appearance of his letters. Attention is focused on meaning, leaving graphic details to habit. A is an excellent subject for the muscle-reader. He reports at times, however, muscular cramp.

While A's writing movements are much freer and smoother than D's, there is some evidence of greater vocal-motor ease for D; thus D's speed in reading aloud is slightly higher than A's, although A was much the more rapid in silent reading. D was able to vocalize mentally more rapidly than could A.

Both reagents show considerable scope of attention<sup>1</sup> and considerable rapidity in work. A's rapidity in executing and synchronizing two processes would seem to be quite exceptional. A great number of tests have shown that he is capable of sustained and effective attention. D's attention is more apt to wander, she was less successful than A in maintaining the double processes at high speed; to some extent this result may be due to her failure to turn over motor details to habit. She maintains a constant supervision over them with this consequence that she makes very few word or letter lapses in comparison with A but when she does make a lapse, it is much more significant of automatism than are A's.

## II. PRELIMINARY EXPERIMENTS

These experiments were tried mostly in the spring of 1913 that we might get some idea of the task in hand and some preliminary practise. At first the reagent simply made loop- or roof-strokes continuously while reading silently. After a few trials both of us were able to do this without any effort. Then the reagent tried taking dictation while reading to himself. The reagent began reading and continued making loops until the experimenter dictated a sentence, a word at a time. He wrote this sentence repeatedly until the experimenter dictated another. The record of reagent A is given to illustrate what happened.

A did not find this task at all difficult introspectively. An

<sup>1</sup> In part, we judge this from their reading-rate. Dearborn writes, "A wider 'spanning' of attention,—as denoted both by the greater frequency of long pauses at the beginning of the line and by fewer fixations per line,—is characteristic of the more rapid readers." *The Psychology of Reading, Arch. of Phil., Psychol. and Sci. Methods.* 1903, 122.

estimate of his reading-rate showed, however, that it dropped from an average normal of 10.67 words per second to 6.32 per second. There appeared to be an increase in writing rapidity just after a sentence was dictated with a gradual decrease in speed and increase in number of lapses as time went on before a fresh dictation. Lapses of letter-repetition and of omission of letters occurred with, also, substitutions of words from a preceding sentence. In one record a whole line was interpolated, made up, probably, from words persisting from preceding dictations. In part, this line seemed written without consciousness of writing it.

It was found that if the sentence dictated was in the form of a question, a definite "set" was aroused with considerable impulse to answer the question. We tried, therefore, a series of tests in which the reagent while reading to himself answered questions asked by the experimenter.

Both reagents reported that with practise it became progressively easier to maintain such a "double set." A, however, succeeded much more completely than D in carrying on a conversation. D, for the most part, wrote very simple and stereotyped answers while A produced elaborate sentences. For both reagents, the speed of reading fell considerably below the normal. A few words with reference to the reactions of each subject may be of value.

Five different tests at five different times were tried with A. His average reading-rate (words per second) under distraction was as follows: (1) 5.27, (2) 4.76, (3) 7.01 (answers relatively simple), (4) 5.72, (5) 4.86. His reading-rate was lower except in (3) than when he was simply taking dictation and much lower than his normal reading-rate. It is, however, a good speed in itself and seems remarkably rapid when taken in connection with the long and involved answers written in answer to the experimenter's questions.

Probably there was tendency for the reading to become more automatic than the writing. A reported that the reading became visual in places and that he got a good idea of the story but had little memory of details. Lapse of consciousness came when phrases or words were repeated while A was waiting for a new question. But, on the whole, there was fairly complete consciousness of what was being written. Breaks occurred when A was "tangled up" in the story. When he was very much interested in the story writing went easily. Many lapses of repetition or omission of letters occurred.

Five tests of the same sort were tried with D also. Her

average reading-rate (words per second) under distraction was as follows: (1) 4.17, (2) 3.02, (3) 4.49, (4) 3.8, (5) 3.21. Her normal reading-rate was about seven words per second. At first, as was said before, D's answers were very simple. She wrote "yes" or "no" if possible; otherwise, "Can't tell," "Don't know" or some other simple reply. As the task became easier with practise she attempted more ambitious answers. Lapses were much less frequent than in the case of A but certain stereotyped ones appear. Thus, "known" is constantly written for "know" without D becoming conscious of the error. "No one" is written for "nothing."

She reported that it was easier to put attention on reading than on writing but that she knew what she was writing. "More like split consciousness than unconsciousness." Answers to questions were first framed in inner speech but there was no need of vocalizing "yes" or "no." Hard questions inhibited both processes. In one place the question was heard indistinctly but answered correctly. In the last two tests there was much evidence of increasing automatism for the writing. D, while still conscious of the words written, lost consciousness of the individual letters. Once or twice she started an answer without knowing what she was going to write. There was less control and more spontaneity.

It is interesting to note that although an interval of more than three months intervened between the second and third test in this series, on account of the summer vacation, there was no loss in skill in manipulating the two processes for either reagent. We found, in general, that the practise-effect so noticeable in the work in the spring had held over when we returned to the experiments the following fall.

### III. SILENT READING AND WRITING

*Method.* In this series of experiments the reagent wrote the given verse ("Thirty days hath September," etc.), continuously until he had finished reading the chapter of a novel. The experimenter shifted the sheets of paper so that only one verse was written on a sheet. The subject held the book conveniently in his left hand, with his right hand in position on the writing-pad on the other side of the screen. At a signal from the experimenter the writing and reading began, simultaneously so far as the reagent could manage it. At the signal for beginning the experimenter started two stop-watches. The whole time required for reading the chapter was obtained from

one stop-watch, the time required for writing each verse was obtained from the other. A and D served as reagents.

Toward the close of this series two normals were taken each day for the speed of writing the verse. One normal was taken with the screen under the same conditions as held during the distraction experiment, the other was taken without the screen under the usual writing conditions. In all normals the highest possible speed was maintained.

Normals for the rapidity of silent reading were also obtained at intervals. No attempt was made to test in detail the memory of what was read under distraction but the reagents were instructed to get the meaning. Both followed the story as in ordinary reading for pleasure without much difficulty. Their memory of the novels read is perhaps, after an interval of some months, more complete than is usually the case with casual reading.

The method here followed does not permit any exact equating of the amount read and written at any particular time. It was possible for reading and writing spurts to occur alternately. In the later work there is, however, a record of the amount read for each verse.

*Results.* The results are given in tabular form in Tables I and II. Table I summarizes the results for the reading of Mrs. Ward's "The Mating of Lydia" from the eighth chapter on. The reading of London's "The Abysmal Brute" and the reading of "The Game" by the same author were each completed in one long session and reading-normals were obtained for alternate chapters. The verses written while the reagent read "The Game" were done in ink, otherwise, the writing was in pencil.

A glance at the tables shows that A both wrote and read normally at a higher speed than did D. For both reagents there is an increase in the speed of the normals with practise. The normal with the screen was found to be slightly more rapid than that without so that the normals given with "The Mating of Lydia" are a trifle too high.

The normal reading-rate also shows a tendency to increase; such an increase was noticed for each story separately. The maximum speed is usually attained at the climax chapter.

The normal reading-rate under distraction shows a fairly constant increase, indicating the practise effect. The writing under distraction also shows increasing speed. D's average speed per verse dropped from 91.33 seconds to 63.66 seconds, A's, from 57.58 seconds to 41 seconds.

In order to assure ourselves of the simultaneity of the two processes, we hoped to get the average speed of both reading and writing under distraction down to the normal rapidity. The experiment, however, consumed so much time that we found it necessary to content ourselves with an approximation of the normal speed or with obtaining a record as low as the normal in an occasional verse. Toward the end of the series A's writing speed approximated the normal very closely. His record speed in this series for both normal writing and writing under distraction is 39 seconds. It should be noted that his reading-speed with the latter record is also high. (See Chap. VI, "The Abysmal Brute.") A's reading-rate under distraction also approached his normal. His introspective reports would seem to show that his reading became more and more visual as the test proceeded.

TABLE I  
SILENT READING AND WRITING

Reagent	Date	“ Mating of Lydia ”  Chap.	Pages <sup>2</sup> Read per Minute		Time of Writing Verse in Seconds				
			Normal	Distraction	Normal <sup>3</sup>	Distraction			No. of times written
						Av.	Min.	Max.	
D	Oct. 10	VIII	.....	.53	.....	89.38	71.0	116.0	16
	Oct. 13	IX	.....	.82	.....	91.33	83.0	107.0	15
	Oct. 15	X	1.62	.....	.....	.....	.....	.....	.....
	Oct. 16	XI	.....	.78	.....	76.56	64.8	90.0	22
	Oct. 16	XII	1.40	.....	.....	.....	.....	.....	.....
	Oct. 17	XIII	1.42	.....	.....	.....	.....	.....	.....
	Oct. 20	XIV	.....	1.01	60.6	78.06	67.0	91.0	17
	Oct. 21	XV	.....	.84	63.2	79.54	64.4	94.0	20
	Oct. 23	XVI	.....	1.00	59.5	76.71	63.4	92.3	17
	Oct. 23	XVII	.....	1.38	.....	71.15	62.0	83.4	12
	Oct. 23	XVIII	1.66	.....	.....	.....	.....	.....	.....
	Oct. 27	XIX	.....	1.14	54.4	71.51	62.4	84.2	17
	Oct. 28	XX	1.64	.....	57.0	.....	.....	.....	.....
	Oct. 28	XXI	.....	1.13	.....	72.40	65.0	79.4	16
Oct. 28	XXII	.....	.95	.....	72.36	66.0	85.4	23	
A	Oct. 13	IX	.....	1.68	.....	57.58	52.0	62.0	12
	Oct. 14	X	1.95	.....	.....	.....	.....	.....	.....
	Oct. 15	XI	.....	1.63	.....	51.02	40.0	57.0	15
	Oct. 15	XII	2.23	.....	.....	.....	.....	.....	.....
	Oct. 16	XIII	1.88	.....	.....	.....	.....	.....	.....
	Oct. 17	XIV	.....	1.75	.....	50.17	42.5	57.0	15
	Oct. 21	XV	.....	1.66	40.0	45.83	41.2	52.9	18
	Oct. 22	XVI	.....	1.80	42.5	48.87	43.6	54.8	14
	Oct. 23	XVII	.....	1.93	41.9	44.57	42.0	46.5	11
	Oct. 23	XXVIII	2.66	.....	.....	.....	.....	.....	.....
	Oct. 24	XIX	.....	2.20	43.5	47.62	44.5	59.0	13
	Oct. 27	XX	2.93	.....	.....	.....	.....	.....	.....
	Oct. 28	XXI	.....	2.31	43.0	44.64	43.0	47.2	13
	Oct. 28	XXII	.....	2.21	.....	42.60	39.0	45.0	16

<sup>2</sup> About 275 words to the page.

<sup>3</sup> Without use of screen.

TABLE II  
SILENT READING AND WRITING  
Reading—"The Abysmal Brute."—(London)

Reagent	Chap.	Normal		Chap.	Distraction				
		Pages <sup>4</sup> per minute	Verse time in seconds		Pages <sup>4</sup> per minute	Verse Time in Seconds			No. of times written
						Av.	Min.	Max.	
D	I	3.00	54.2 <sup>5</sup>	II	1.30	66.65	61.6	78.6	15
Nov. 11	III	2.90	.....	IV	1.66	69.98	68.0	73.2	5
	V	2.61	.....	VI	1.84	71.52	65.8	81.0	16
	VII	2.69	.....	VIII	1.60	71.91	68.6	73.0	7
	IX	2.19	.....	X	1.70	64.27	59.0	67.8	14
A	I	3.36	39 <sup>5</sup>	II	3.43	42.2	40.2	44.0	9
Nov. 6	III	4.07	.....	IV	3.44	43.7	40.0	46.0	4
	V	3.97	.....	VI	4.23	41.0	39.0	47.5	12
	VII	4.48	.....	VIII	4.67	42.2	41.0	43.8	4
	IX	5.05	.....	X	3.73	42.2	40.0	45.0	10

Reading—"The Game" <sup>6</sup>—(London)—Ink Records

D Nov. 20	II	2.84	61 <sup>5</sup>	I	1.25	70.2	61.8	87.2	11
	IV	2.59	.....	III	1.43	63.66	60.0	73.0	6
	VI	2.81	.....	V	1.32	65.66	57.0	71.0	12
A <sup>7</sup> Nov. 18	II	4.22	42 <sup>5</sup>	I	3.92	43.75	43.0	44.5	6
	IV	3.30	.....	III	3.77	45.8	44.0	47.0	3
	VI	4.42	.....	V	3.41	45.3	43.0	50.0	7

<sup>4</sup> About 149 words per page.

<sup>5</sup> With screen.

<sup>6</sup> About 132 words per page.

<sup>7</sup> Bad physical condition.

*Introspections.* The introspections (more accurately retrospections) given by the reagents are fairly adequate. Both reported, however, that any assumption of the introspective attitude in the course of the experiment complicated conditions badly. At times, in spite of concentration upon the reading, the thought would come "How am I doing this?"

For D another complication entered occasionally in that descriptive bits in the stories read suggested concrete visualizations. An attempt to maintain the three processes, reading, writing, and concrete imagery, caused considerable retardation. In time D was able to inhibit somewhat the tendency to visualization. She had also to inhibit a tendency to pause and think over any generalizations in the stories.

In order to make it possible to trace in some detail the course of the experiment we are giving extracts from the daily introspections chapter by chapter.

First, for D. "The Mating of Lydia," VIII—"All verse-words were mentally heard or vocalized, but as the reading



went on the vocalization became lighter, as it were." IX. "Auditory-vocal-motor cue for every word written. Sometimes this word seems to float over the reading consciousness. Complete loss of orientation of hand, very distressing." XI. "Process getting more automatic. Writing went easily. Auditory-vocal-motor cue for every word but not much kinaesthetic report except of a blurred sort." Certain stereotyped lapses appear as a doubling of the first stroke of "y" and the doubling of "v." D had the feeling of writing these letters correctly. XIV. "Writing went easily. The auditory-vocal-motor cue for verse floats over the auditory-vocal-motor reading. Lost report from 'y's' and 'i's'." Alignment lost completely after fourth record. XV. "Verse went easily after once started. Word by word cue. Toward close the kinaesthetic report began to lapse." A curious lapse appeared at this point. In writing the eighteenth verse D omitted the fourth line. In writing the next verse D repeats this line. D reported neither of these lapses. Toward the close of the nineteenth verse, D reported that she "came to" with a feeling that the word "April" had been written last, although at that moment she was writing another line. She concluded, incorrectly, that the second line had been cut. XVI. "The verse cue is auditory-vocal-motor, word by word. I believe that this word is mentally vocalized during breaks in the reading and that it then echoes in consciousness until written. Perhaps toward the close of each chapter synchronization of reading and writing occurs." XIX. "More automatic than before. Kinaesthetic report goes first. Feeling of irresponsibility as to details. Auditory-vocal-motor cue remains but seems to float over the reading. Sometimes there was an actual doubling of consciousness. Cue was more schematic than before. Meaning dropping out, numerals particularly unmeaning." XXI. "Reading becoming easier and easier to maintain, and writing more automatic. Writing cue persists but is more and more attenuated. Meaning evaporating from verse so that I can't hold my place. Kinaesthetic details gone." XXII. "The reading seems in part visual, in part, the two vocal-motor processes are synchronous. Meaning is evaporating; the lines and words are becoming detached. Lapse of kinaesthetic report very marked. Loss of orientation very distressing." D reported that she would not have been surprised to find lines omitted from the verses. None were omitted.

"The Abysmal Brute." II. "Lost space-relations. No idea where writing is." VI. "Lost connections between lines and

lost meaning of words even while writing them." VIII. "Lost connections within line, puzzled as to what has been written." X. "The writing for this chapter is much more AUTOMATIC than for any other chapter. Loss of strain in arm, usual blurring of kinaesthetic report. The first 'T' is now merely a signal for setting off reaction, it is not thought of as a 'T.'"

"The Game." I. "Distressing. Felt as though the pen were making no record. Kinaesthetic report inadequate and defective." III. "Easy smooth movement but lapsing of all kinaesthetic details." V. "Writing went easily. Verbalized verse throughout."

Secondly, A's introspections. "The Mating of Lydia." IX. "Consciousness of verse high at beginning, then lapsed. Vocal-motor cue throughout but this cue appears to be both marginal and imaginal." XI. "Vocal-motor cue throughout for verse but a rapid running together of words." XIV. "Lost consciousness of writing at one point in reading." "Came to" and found himself writing last line of verse. XIV. "Excessive spurring, excessive movement, many flourishes." XV. "Fairly conscious of verse with verbal cue; second, third, and fourth lines most apt to drop out. Consciousness reduced for these lines." XVI. "Exciting chapter. Vocal-motor verse-cue, line by line. Attention on reading rather than on writing." XVII. "Cue for verse is line by line, for each verse conscious of cue for only two lines, but these lines vary from verse to verse." XIX. "Chapter very interesting, main interest in story." XXI. "Attention on reading throughout. Can't tell about writing-cue. Perhaps a line-cue. Whole line verbalized. Writing and verbalization not synchronized. Very little attention on writing."

"The Abysmal Brute." II. "Cue as for normal writing. Hand report not detailed, only general movement felt." IV. "Attention largely on reading. Toward close of test writing may have become automatic." VI. "Chapter less interesting than before. Writing was less automatic." VIII. "The kinaesthetic report drops out of consciousness. If a word is pronounced, it feels written. General feel of movement is of up and down strokes. Chapter is intensely interesting. Climax of story." X. "Attention mainly on reading. Cue more pronounced at beginning than at close of verse. Probably unconscious of any cue on verse eight or nine."

"The Game." A was in bad physical condition, feverish. I. "Had a little trouble getting into the story because of the abrupt opening. Attention mainly on reading. The kinaesthetic report not detailed, only big sweeping strokes came to

consciousness. Reading was largely visual, a paragraph was taken in at a time with verbalization of occasional words only. No memory of any verbalization of verse, cue gone more completely than ever before, process more automatic." A found that the strain in his hand and arm and the pain in his head (due to condition) vanished from consciousness during the test. III. "Attention came to the writing, there were vocal-motor cues throughout, the reading was largely visual." V. "Interest in reading high. Emotional interest. Attention off writing but would revert to writing suddenly and unexpectedly. Less visual reading than before."

A reports, in general, in regard to the verbal cue for writing the verse, that only when he definitely thinks to notice whether or not it is present at the time of writing is he aware of it. Otherwise memory does not avail to tell whether or not such a cue was present.

#### IV. SIMULTANEOUS WRITING AND ADDING

*Method.* This experiment was similar to those above, except that the adding of columns of figures was substituted for the reading. Instead of the verse "Thirty days," etc., the two verses beginning "Little drops of water," etc., were written.

The method of procedure was similar to that in the above experiments. The subject held the slips with the columns to be added in his left hand, while writing the stanzas with his right hand, extended through the black cloth screen. The experimenter, on the other side of the screen, observed the writing process and recorded the time. The subject wrote the two stanzas of this selection through once, adding as many of the columns of figures as possible. The experimenter took the time for writing the whole verse, for adding each column and for adding each problem, making note also of any breaks that occurred in the writing and noticing particularly the effect of announcing results.

Each problem added consisted of nine three place figures. The first 24 problems were taken from the Curtis Standard Arithmetic Tests No. 1. Later, problems were made by changing the order and transferring numbers given in the first 24. These later problems were prepared on an adding machine, the totals being turned under.

Only one subject was used in this test, Subject A. Subject D tried the test several times, but had such difficulty in performing the double operation that the test with her was abandoned. The experiment ran for twelve days, with, at

times, several days intervening between the tests. The longest break was five days, between the first and second tests. A normal writing record for the verse was obtained, both before and after each test, one without and one with the screen. The normals with and without the screen were alternated in order that neither normal might constantly have the benefit of the practise and warming up. On days that ink records were obtained, ink normals were used.

After completing the writing of the two verses, a very full introspective report was obtained from the subject. This introspective report covered the kind and amount of writing cue, the memory of lapses, confidence in the accuracy of adding, and such other things as seemed worthy of comment. At the conclusion of each test, the results of the additions were checked column by column.

At the beginning of the experiment, the subject was able to add only two problems while writing the stanzas once, then with a slight reduction in adding time, he added three problems (which tended to lengthen the writing-time of the stanzas) and then, after several more days, with a great decrease in the writing time, he was able to add two problems only. From the point of return to the two problems to the end of the experiment, only two problems were added during the writing of the stanzas, as both writing and adding times decreased almost simultaneously.

It was found necessary, in order to make the tabular representation of results comparable, to take two-thirds of the total writing time as the basis on which to draw up the tables, in those cases where three problems were solved.

Normal adding times were obtained on four different days.

*Results.* During the course of the experiment, the writing time for the two stanzas decreased from 122" obtained on the first day to 46" obtained on the last day. The highest adding time for all problems was 78" on the third day and the lowest 18" obtained on the last day. Both of these records were obtained for the first problem. For the second problem, the highest record was 60" on each of the first, second, and third days, and the lowest was 24" on the ninth day. For the third problem, the highest was 65" on the fifth day and the lowest 35" on the same day.

The average adding time under distraction for the first problem was 31.34", for the second, 38.27", for the third 48.60". A third problem was utilized only a few times during the test, so that this high record is not significant. Throughout the experiment, however, the time for adding the second

problem is consistently higher than for adding the first, and the time for the third is higher than for either of the other two.

The average adding time for twenty-one problems under normal conditions was 19.8" with a maximum time of 27" and a minimum time of 15". All of the normal additions were correct. Among the additions performed under distraction, we find that for the first problem 19 were correct and 33 wrong; for the second problem, 17 were correct and 33 were wrong; for the third problem 5 were correct and 10 were wrong; and for the fourth problem, 1 was correct, making in all, 42 correct and 76 wrong additions out of 118 problems. The number of correct additions made is indiscriminately mixed with the number of incorrect, except for one day, the fifth of the test, when eight problems out of thirteen were done correctly.

The average time for the normal verse writing, when the screen was not used, was 46.88" and when the screen was used 45.41". As before, a constant tendency for the writing with the screen (that is, when visual control was absent) to be lower than for the writing without the screen (when visual control was present) was noticed. The average normal time for writing the verse (speeded) with and without the screen was 46.22". The highest normal writing, without the screen, was 53" made on the 5th day and the lowest 42.5" made on the 8th day. The highest normal time with the screen, was 50" made on the first day, and the lowest 40" made on the 12th or last day.

The following table, Table III gives in detailed form the course of the writing side of the experiment. The first two

TABLE III  
WRITING TIMES FOR VERSE

Date	Normals		Distraction			
	Without Screen	With Screen	Times Written	Average	Minimum	Maximum
Nov. 26.....	50.5	50.0	1	122.0		
Dec. 1.....	50.4	49.9	2	104.5	97.0	112.0
Dec. 2.....	50.0	46.0	4	96.75	83.0	121.0
Dec. 4.....	50.0	49.0	5	97.40*	86.0*	107.0*
Dec. 5.....	50.7	48.0	3	80.3*	75.0*	86.0*
Dec. 8.....	45.0	46.5	3	71.0*	64.0*	75.0*
Dec. 9.....	45.2	44.0	4	72.75*	66.0*	89.0*
Dec. 11.....	42.75	46.0	7	75.14	63.0	93.0
Dec. 12.....	46.8	44.0	5	63.30	52.0	78.0
Dec. 15.....	43.0	42.5	6	57.33	49.0	66.0
Dec. 16.....	44.0	40.5	7	54.21	49.0	63.0
Dec. 17.....	43.0	40.0	5	49.0	46.0	54.0
General Averages..	46.78	45.53		78.64		

columns show the gradual decrease that was made in the normal writing times, and the last three columns the decrease in the writing times under distraction.

Table IV presents the results that show the gradual decrease in the adding times of the various problems. The last three columns give the general results of all the problems.

TABLE IV  
TIMES FOR ADDING PROBLEMS

Date	Times Verse Written	1st Problem			2nd Problem			3rd Problem			All Problems		
		Av.	Min.	Max.	Av.	Min.	Max.	Av.	Min.	Max.	Av.	Min.	Max.
Nov. 26	1	59			60								
Dec. 1	2	51	47	55	52.5	48	57				51.7	47	57
Dec. 2	4	46	34	78	50.7	49	60				48.3	34	78
Dec. 4	5	35	31	40	51.6	34	60	60	65	53			
Dec. 5	3	34	27	40	40.6	35	50	44	49	35	39.5	27	50
Dec. 8	3	30	30	30	34.3	30	43	44.3	50	37	36.6	30	50
Dec. 9	4	40	28	53	33.2	31	37	41	44	39	38	28	53
Dec. 11	7	32.4	25	43	42.4	35	50				37.4	25	50
Dec. 12	5	26.2	25	28	35.4	24	50				30.8	24	50
Dec. 15	6	25.8	20	33	30.5	25	36				28.1	20	36
Dec. 16	7	23.1	20	28	31	26	40				27.0	20	40
Dec. 17	5	20.6	18	25	28.2	26	33				24.4	18	33
General Averages		35.3			40.9			47.3			36.6		

The next table V gives the results of the normal adding times. The results in this table are to be compared with those given in the preceding table for the adding times under distraction.

TABLE V  
NORMAL ADDING TIMES

Date	Problems Added	First Problem			Second Problem			All Problems		
		Av.	Min.	Max.	Av.	Min.	Max.	Av.	Min.	Max.
Dec. 1	3	22.6	20.8	24				22.6	20.8	24
Dec. 9	2	16.0	15	17				16.0	15	17
Dec. 11	4	19.5	18	21	24.5	24	25	22.0	18	25
Dec. 16	12	17.3	16	20	20.6	17	27	19.0	16	27
Averages		18.6			22.5			19.9		

Tables III and IV indicate gradually increasing proficiency in handling two processes simultaneously. Inasmuch, however, as the record normal writing time for the two stanzas was 40" and the record writing time under distraction was 46" (both made on the same day) it is evident that the two processes never became completely synchronous. On the

adding side, the record normal adding was 15" and the record adding under distraction 18" (made on different days—16" and 20" were records made on same day). In the case of the writing the difference is 6" and in the case of the adding 3". The difference in the writing speed is the time taken to vocalize the verses.

*Introspections.* Extracts from the introspections made by the subject in the course of the experiment will serve to indicate the process gone through during the experiment.

First, a general summary of the introspections on the normal writing. These concern mainly the cue used for writing. "The cue is definitely vocal-motor, largely word by word, prolonged during the writing, the next word being verbalized just as the last stroke of the preceding word is being finished. There is a certain foreflitting of words that is too shadowy to serve as a cue. The unit for meaning is line by line, while the unit for the cue is word by word or, where words are very short, phrase by phrase. This makes it difficult to introspect the cue, as the meaning sometimes makes it seem as though it were coming line by line. When thinking about the cue, there is a tendency to cut the cue unit." From the beginning of the taking of normal writing to the end, a constant tendency for the verse cue to become less and less distinct is apparent.

The introspections concerning the cue made on the writing under distraction, are, in brief, as follows: first day,—“Verse verbalized completely, with the last words of each line verbalized a second time; for instance, the 4th line was first verbalized as a whole and then ‘pleasant land’ was repeated about the time I reached ‘pleasant’ in the writing;” second, third and fourth days,—“Vocal-motor cue for the verse;” fifth day,—“Cue for writing drops out in places. Writing report is not clear;” sixth day,—“Vocal-motor cue less noticeable than before. Grapho-motor report is lost;” seventh day,—“Vocal-motor cue definite but not very clear;” eighth day,—“Vocal-motor cue for writing more blurred than before. Some unawareness of writing but vocal-motor cue for verse throughout;” ninth day,—“Vocal-motor cue for verse only at intervals. No remembrance of writing cue as a whole.”

The introspections concerning the synchronization of the two processes are self-explanatory. They show, also, the gradual development of proficiency in handling the complex situation.

First Day. “Attention mainly on addition. Some flickering

of attention. Adding seems to be in part visual. Conflict between two activities is largely on a vocal-motor basis."

Second Day. "Two processes were not synchronous, although in the first verse slight tendency to synchronization is noticed. While writing, the adding was merely held in mind, except once or twice where small figures were added. Attention is largely on the adding. At certain places, doubling of the vocal-motor process seems to take place."

Third Day. "Attention more on adding than writing. Adding is in part visual, in part vocal-motor. Number of figures taken in by the eye at one time is growing. Habit of making additions without revision is forming. Writing comes in the adding breaks. Pause in adding comes after the first three numbers in the columns. Processes do not seem at all synchronous."

Fourth Day. "Considerable fluctuation of attention. Hand is never forgotten except when not moving. Adding seems more visual than before. At beginning of the column the writing and adding seem synchronous, then as adding becomes more difficult, the writing is stopped." (Experimenter's observation, "No writing of verse while A is speaking.")

General Introspection. "I start writing the verse first, giving a definite vocal-motor cue for the first line, then when the hand is on its way, start the adding, which at first runs along with the writing, both having a vocal-motor cue. The numbers are a mere flicker or shadow above the well-defined writing cue. But as the process grows more difficult, it tends to take over completely the entire vocal-motor process and the writing stops. When I get to the end of the column, the adding being over and the result announced, I give a second definite vocal-motor cue for the writing. Again the two processes run on synchronously for a while, with the adding gradually taking up the entire process. When I get through adding the first problem, I become conscious of having written only a small part of what should have been written, and spurt, paying a little more attention to the writing than to the adding. Then again the adding comes to the fore."

Fifth Day. "Synchronization never occurs for more than half a column. Attention is largely on the adding but there is a little more synchronization than before. Attention fluctuates. Practise effect is beginning to show on the adding. Adding has become more visual. Writing half drops out in places."

Sixth Day. "Processes are more synchronous than before. Adding is becoming visual. Much synchronization. Writing tends to drop out and attention goes to the adding."



Seventh Day. "Very difficult to-day." 1. (first writing time). "Processes don't synchronize." 2. "Slight synchronization." 3. "Processes synchronized throughout the first verse and first two lines of second. Visual adding is not noticeable. Unit of addition is growing bigger."

Eighth Day. "Processes well synchronized. Writing and adding went on together. Some unawareness of writing. First problem was well synchronized with writing."

Ninth Day. "Attention chiefly on adding. Some fluctuation of attention. Lapse of hand-awareness for long stretches. Hand innervation comes more easily than earlier in test."

Tenth Day. "Went very easily. Synchronization throughout. Adding seems a trifle more visual than before. Large adding units are taken in at a glance."

Eleventh Day. "Very little attention on the verse." (Experimenter's observation, "Results now announced while A is writing.")

#### V. READING ALOUD AND WRITING

*Method.* The conditions under which this test was tried were, for the most part, similar to those of the third section except that the reagent read aloud instead of reading silently. In order to test the memory of what he had read the reagent stopped writing at the close of every verse, or, on some occasions, at the close of every third or fourth verse and gave what he could recall of the contents read. He also gave his introspections and reported any lapses in the writing that he remembered. Breaking the writing at frequent intervals made it possible to determine the exact number of words read during any particular period, and insured a fairly accurate equating of the amount read and the amount written at any particular time. In this respect the records are more satisfactory than those of the second section. Moreover, since the introspections were given at shorter intervals they are probably more adequate than before, although the frequent breaks may have prevented a gradual absorption in the reading and contributed to a heightened capacity for handling the double process through extreme concentration of attention for a short period.

As before, the speed with which the verse was written was timed. Normal reading-rates were obtained at the beginning and toward the close of each session. Normals for writing speed we obtained one at the beginning, the other at the close of the session, alternating as first or second the normal written with the screen and the one written without the screen.

The experiment ran for twenty-three sessions, Subject A served as reagent.

*Results.* The growing ease with which A handled the situation is indicated by Table VI which shows, first, the increase in the number of words read per second, and secondly the gradual increase in the rapidity with which the verse was written; a rapidity, toward the close of the experiment, approximating the normal writing-time.

TABLE VI  
READING ALOUD AND WRITING. REAGENT A

Reading	Date	Words read per second				Time of writing verse				
		Normal	Distraction			Normal <sup>8</sup>	Distraction			No. of times written
			Av.	Max.	Min.		Av.	Min.	Max.	
"The Intruder"	Dec. 18	No record								
Scientific reports	Dec. 19		2.31	2.90	1.92	39.0	71.21	49.5	111.0	7
"	Jan. 12	"	2.43	2.73	2.03	38.5	63.83	56.0	72.0	6
"	Jan. 13	"	1.67	1.98	1.48	41.0	80.37	72.5	90.0	4
"	Jan. 13	3.33	1.74	2.36	1.35	40.5	63.50	57.0	66.5	10
"	Jan. 14	3.38	1.83	2.12	1.50	39.5	61.90	60.0	65.0	5
Phillip's "Genesis of Number Forms"	Jan. 15	3.38	2.16	2.55	1.81	42.0	62.35	56.0	71.0	10
"	Jan. 16	3.45	2.38			41.0	58.00	53.0	72.0	8
"	Feb. 3	3.55	2.27	2.58	1.95	41.0	64.29	56.5	71.4	7
"	Feb. 4	3.33	2.03			40.7	55.29	44.0	67.5	8
Sanford, "Psychic Research in the Animal Field"	Feb. 5	3.34	2.39	2.65	2.13	38.4	55.09	50.0	61.0	9
"	Feb. 9	3.53	2.15	2.40	2.01	40.0	55.73	51.2	58.1	7
"	Feb. 10	3.91	2.48	2.59	2.36	36.0	51.55	48.0	54.0	11
"	Feb. 11	4.00	2.84	3.13	2.53	38.4	46.65	43.2	52.0	7
"	Feb. 13	3.76	2.61	2.91	2.30	35.5	47.78	43.0	55.5	6
"	Feb. 16	3.63	2.58	2.96	2.36	38.0	49.71	44.7	53.6	6
"	Feb. 17	3.83	2.85	3.03	2.40	33.7	44.65	42.4	46.5	7
"	Feb. 18	4.13	3.21	3.55	2.91	36.7	43.50	40.7	46.8	6
"In the Wireless Room" (story)	Feb. 19	4.26	3.07	3.43	2.84	35.8	47.95	45.0	52.5	6
"	Feb. 20	4.03	3.54	4.25	2.89	36.0	43.57	40.1	45.4	7
"	Feb. 24	4.25	3.24	3.83	2.65	38.0	43.15	40.5	46.0	8
"	Feb. 25	4.17	3.34	3.89	2.80	34.2	41.12	37.1	45.4	9
"	Feb. 26	4.71	3.75	4.00	3.32	36.1	39.19	35.0	43.8	9
"	Feb. 27	4.91	3.62	4.01	3.13	33.0	37.96	34.5	45.5	5

<sup>8</sup> Speeding with screen.

*Introspections.* On the introspective side there was reported a gradual increase in the ease with which the double process could be maintained. At first there was very little synchronization of processes. Breaks would occur in the reading, during which A would get his vocal-motor cue for the writing. These fluctuations in attention were very rapidly and skilfully executed. Frequently, A spoke of getting a general "set" for the two processes before beginning writing or reading.

A bad initial "set" caused breaks in the process, so, too, any interference while the writing was in progress. Such interference occurred when there was difficulty in getting the

meaning of what was read, when there was difficulty in pronouncing a word, when the paper slipped however slightly, or when the experimenter touched A's hand. On several occasions the entrance of someone into the room where the experiment was being conducted caused bad breaks in the process. These breaks were evidenced not only by A's report but also by the slowing of the time of writing and, usually, by a repetitionary lapse. Such breaks show also the tension created by the situation. Greater ease in writing the first three lines seemed due to persistence of initial set through that point. After that there was tension.

At the fifth session A reported that the kinaesthetic "report" was lapsing. From this time on, there were reports of increasing ease in handling the situation, there was also increasing excellence in the memory of reading-contents. A's reliance upon the kinaesthetic report was shown by the difficulty he had in handling the second and third lines, both of which commenced with a capital a. A repeated feeling of doubling or omitting one of these lines seemed so determined.

The experiment ran a long time before A reported any synchronization of the two processes. The growing ease in handling the situation seemed due, not to synchronization, but to the fleeting character of the vocal-motor writing cue. In time, too, the cue was cut in that one word or letter could serve as the cue for several words. Not until the thirteenth session did A report synchronization of processes. He began to describe the writing cue as floating over the reading. In the course of this session the writing became "fast and furious." At the next session A began to complain that meaning was evaporating from the verse he was writing, so that if a break occurred he had difficulty in picking up his cue. Later still he reported, "Cue seems to come after not BEFORE the word is written. It is definitely on top of the reading process." Again, "The two processes are definitely synchronous. Lines seemed shoved together, telescoped. Not enough words. What's gone?"

The reading varied from expressive reading to reading that was very monotonous or breathy. With a shift in style, as one article was changed for another, there was for a time increased difficulty in handling the problem which indicated that in part attention-habits, induced by familiarity with style, were operative in increasing speed of reading. The normal reading-rate also showed a progressive increase, not only within the scope of a single article, but also in the experiment as a whole.

The final experiments in this section were carried on while A was reading a very interesting story. Toward the climax of the story there was a strong inclination to stop writing altogether.

## VI. READING ALOUD AND WRITING FROM DICTATION

*Method.* There was little change in method. As before the reagent sat to the left of the screen with his arm, extended through the sleeve, resting on the writing-pad, while the experimenter, on the other side of the screen, manipulated the writing-sheets, dictated material, and timed the reading and writing. The reagent read aloud continuously from a story while writing from continuous dictation, words given, that is, one at a time as rapidly as the reagent could write them. At the close of set intervals, varying from one to three minutes, with an occasional five minute period, the reagent was stopped and the experimenter recorded (1) the subject's memory of the contents read, (2) his memory of the matter dictated, (3) introspections as to the course of the experiment. In the second half of the experiment his memory of the contents dictated was recorded first and his memory of the contents read taken after that. Record was made of the number of

TABLE VII  
READING ALOUD AND WRITING FROM DICTATION  
Reagent D

Story	Date	Normal	Distraction			
		Average number of words read per second	Average number of words read per second	Average number of words written per minute	Average number of words remembered	Length of writing interval
R. H. Davis' "In the Fog"	Mar. 30	4.03	3.07	20.00	3.0	1 min.
	Mar. 31	3.83	3.18	19.85	3.1	1 min.
	Apr. 1	4.86	3.46	16.25	1.5	3 min.
	Apr. 2	4.75	3.63	18.22	1.4	3 min.
	Apr. 3	4.91	3.98	17.44	2.1	3 min.
	Apr. 6	...	3.53	15.88	1.7	3 min.
	Apr. 7	5.00	3.86	17.90	.9	3 2(5)
	Apr. 8	4.50	3.67	15.94	.7	3 4(6)
Davis' "A Derelict"	Apr. 27	4.08	3.13	18.58	1.2	3 min.
	Apr. 28	3.83	3.32	16.73	1.1	3 min.
	Apr. 29	4.01	3.43	18.06	1.2	3 min.
Davis' "La Lettre D'Amour"	Apr. 30	4.18	3.76	18.94	.9	3 min.
	May 4	4.95	4.00	20.77	1.6	3 min.
Davis' "The Man With One Talent"	May 5	5.18	3.98	17.83	1.0	3 min.
	May 6	5.76	4.60	21.22	2.7	1 min.
	May 7, a.m.	5.35	4.92	22.85	3.4	1 min.
	May 7, p.m.	5.65	4.66	22.33	1.3	1 min.

words read aloud and of the number written from dictation each minute. At the beginning of the session and sometimes toward the close a record was made of the normal rate of reading aloud. Both A and D served as reagents.

*Results.* As before, the tables (VII and VIII), show the effect of practise upon the reading-rate. There is a fairly constant increase in speed for both the normal reading and that under distraction. The introspective records show that subjectively, the reagents found the experiment an exceedingly easy one to carry out but in spite of this the reading with distraction failed to reach the normal speed. The number of words written per minute<sup>9</sup> is well under that which can be taken in normal conditions, and, curiously, there is little evidence of any tendency to an increase in the number of words taken per minute. A's record would seem to indicate a tendency to decrease, D's record at the point of greatest reading speed is higher than the average. Throughout, the record for the first minute is consistently higher than that for subsequent minutes, as though during the experiment the reagent "ran down." Both reagents had a good memory of the contents read but A's report was considerably more detailed than D's. There were times of curious lapse of memory of details, both reagents reported a feeling of having them just on the tip of the tongue and then losing them. A's memory of the

TABLE VIII  
READING ALOUD. WRITING FROM DICTATION  
Reagent A

Story	Date	Normal	Distraction			
		Average number of words read per second	Average number of words read per second	Average number of words written per minute	Average number of words remembered	Length of writing interval
Davis' "The Bar Sinister"	Mar. 2	4.66	2.28	22.7	9.1	1 min.
	Mar. 3	4.02	3.20	19.8	9.4	1 min.
	Mar. 5	?	3.26	25.0	7.0	1 min.
	Mar. 6	4.58	3.19	19.3	1.0	3 min.
	Apr. 9	4.66	3.88	19.5	1.5	3 min.
Downey's "The Ghostly Sting"	May 7	4.60	3.70	21.0	7.1	1 min.
	May 8	4.40	3.65	19.2	10.0	1 min.
	May 11	5.3	4.48	17.5	6.1	1 min.
Davis' "The Editor's Story"	May 12	5.15	4.02	18.3	6.0	1 min.
	May 14, a.m.	5.00	4.46	17.2	5.2	1 min.
	May 14, p.m.	5.06	4.49	17.6	5.0	1 min.

<sup>9</sup> The number of letters written per minute was also recorded, but to both reagents the word appeared to be the actual unit.

words written was much more complete than D's. It is significant that just about as many words could be recalled at the close of a one minute interval as at the close of a three minute or longer interval. D lost, or failed to make, connections between words much more significantly than A. On one occasion when the experimenter dictated an interesting bit A repeated the whole (twenty-one words), without a mistake, at the close of the writing. He even criticized the use of certain terms by the author of the story. It is true that his reading-rate on this occasion was somewhat retarded and his memory of the contents less complete than usual. When the experimenter tried dictating a sentence twice, A noticed this and gave many of the words that had been redictated. D, on the other hand, failed to recognize the dictation of such a familiar line as "My country 'tis of thee," a failure which clearly evidenced the absolute lack of connection between the words dictated.

*Introspections.* Both reagents found this task a very simple one, requiring little effort, but from the first there were some differences in their reports. D failed completely to make connection between the words dictated although reporting that while writing each word she knew, with rare exceptions, what it was. At the close of each interval she seldom recalled more than four or five isolated words. A, on the other hand, in the earlier sessions remembered many more of the words dictated, there were occasions when he had an exact memory of both what was read and what written. Moreover, in the first tests at least, he caught the drift of the story that the experimenter was dictating to him.

At first, A repeated after the experimenter the words that were dictated, later such a tendency to repetition dropped out, on some occasions a verbal flicker (vocal or imaginal) seemed to follow the dictation. D reported no such tendency to repetition of words dictated but noted occasionally a faint visual flicker of a word written. Both subjects reported that the experimenter's voice sounded faint, far-off, muffled, even when the experimenter was speaking very loudly and emphatically.

Throughout these tests A reported, "I get the words, as they come, in their connections but they are lost very quickly." Toward the close he caught only phrasal connections. As A reached the climax of a story there was loss of writing control and failure to get the connection between words but the writing did not become completely automatic. When greatly interested in the story there was some tendency for A to stop

writing, the experimenter had to dictate very loudly and at times had to dictate a word several times with great emphasis before it would be taken. Sometimes A reported that a word was heard and written, and that was all there was to it. There was, however, a tendency for him to become alert to the writing. This occurred when the subject-matter especially interested him.

D's reports are more important from the standpoint of automatism. A few extracts from her reports may be given. March 30. "No idea what the story dictated is about." March 31. "Part of the time a feeling of divided attention. Sometimes, half lose the experimenter's voice." April 1. "Words written fuse with story. Once or twice words were written automatically. Difficult to introspect. Lost consciousness of hand once or twice. Extraordinarily little consciousness required for writing. Words dictated often call attention to the writing, so too do any unusual words." April 2. "Lost consciousness of writing. Just get voice." April 8. "There are times when writing is pretty nearly automatic, times when I do not know what words are written." April 27. "Drop in kinaesthetic report. Feeling of irresponsibility as to details." April 28. "Very little writing consciousness but I know the words while writing them. Sometimes lost meaning of word dictated. In places a tendency for the reading to become automatic." April 29. "Words have no meaning. Wrote a few words without return report from hand." April 30. "Feeling of complete uncontrol. (Writing is indecipherable.) Forget words while writing them." May 4. "Can't spell words dictated, they vanish too soon." May 6. "Tendency to forget words in middle, memory seems to hold over about four letters." May 7. "Heard word and wrote it, nothing more. Some very easy words seem to be written unconsciously."

## VI. A SUPPLEMENTARY EXPERIMENT

### *Silent Reading and Writing from Dictation*

Because of the fact that our reports showed so much more evidence of conscious control than did the report of Solomons and Stein<sup>10</sup> under somewhat similar conditions, we tried a supplementary test in which we attempted to conform to their conditions. We substituted silent reading for reading aloud, we gave up speeding, we let the writing run on for long inter-

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<sup>10</sup> L. Solomons and G. Stein, *Normal Motor Automatism*, Psychol. Rev. III., 1896.

vals until fatigue set in, we slowed the rate of dictation, instead of making it continuous, between dictations we fell back on meaningless strokes, we no longer called for reports on the memory of what had been read. No significant change occurred, however. The experiment simply took less effort than before. Although D continued such writing for nearly two hours (two sessions) there was no report of greater automatism than in the preceding test. On the last day, when she was feeling unusually fit, she remembered more of the words written than ever before, recognized redictated sentences, and that without the slightest consciousness of effort. The most significant lapses under these conditions occurred in the case of A, as follows: first, his failure at times to take a word until it had been dictated several times; second, long retardations in taking words without, however, a redictation; third, the repeated writing of a word although it had been dictated but once, because, as he explained, he could not remember whether he had written it at all.

## VII. GENERAL SUMMARY AND CONCLUSIONS

### 1. *Normal Rapidity*

As the experiment continued there was a continuous increase in the normal rapidity for all operations tested, so that the question arose whether we could count on having ever reached a permanent record. This increased speed in the case of reagents already practised in the operations in question is interesting. It depended undoubtedly upon the effort put forth, the definite intention to speed to our limit. As Book and other investigators of the learning process have shown, effort of attention makes for greater efficiency.

*Writing Normal.* For reagent D the time required for writing the first verse ("Thirty days," etc.), fell from 60.6 seconds to 54.2 seconds. These normals were taken without the screen and in pencil. They represent little practise except in so far as the writing of the verse under distraction served to lower the normal time record. This is a good speed, equivalent, for the verse written, to more than two letters per second.

For reagent A we have at hand a much greater number of records both with and without the screen. The slowest record without the screen for thirty trials, on thirty different days, was for the first verse 44 seconds; the fastest, 33.5 seconds. With the screen, the maximum was 42 seconds and the mini-



mum 33 seconds. The average of the last six trials with the screen was 35.5 and without the screen 36.7. These records are very rapid ones, from over three to more than four letters per second. The best records would be made unexpectedly and then, in time, this record would become an average one. Such increase in efficiency even after the limit would seem to be reached is common to all practise curves. It is probable that A reached his physiological limit in speed; he had long passed the limits of legibility. The verse-cues served to release a series of movements that were cut in every possible way. A tried, deliberately, methods of shortening his time; he tried, for instance, the effect of decreasing the space between lines, diminishing letters, etc. The fact that the record with the screen stayed pretty constantly below that made without the screen shows the curious inhibition from the visual report even before the limits of legibility were passed. With the second verse written we have again an increase in speed with practise, with a higher speed achieved with, than without, the screen.

*Reading-normals.* D's rate in silent reading estimated from thirteen normals varied from 6.3 words per second to 7.4 words per second. A's, estimated from the same number of normals, varied from 8.6 to 12.5 words per second. Both records are good (A's unusually so) if we may judge by comparison with the reading-rate for twenty-eight persons given by Huey, who writes, "When the silent reading was at maximal speed, the rates ranged from 3.5 to 13.5 words per second."<sup>11</sup>

The reading-speed, as the writing-speed, showed a tendency to continuous increase, but this tendency was complicated by two other factors: (1) The effect of the contents read upon the speed of reading, with great rapidity at points of climax, and (2) the effect upon reading-rate of familiarity with a writer's style.

The general tendency toward increase in reading-rate seemed in D's case due to inhibition of certain general habits. Many things have operated to reduce D's speed in reading such as close grammatical work on the classics, critical reading, correction of students' themes. She developed during the practise some power of inhibition of a habit of criticizing details and pausing over generalizations. She also inhibited, in part, a tendency to indulge in concrete visualization of descriptive bits. A's increase in speed would seem due, in

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<sup>11</sup> "The Psychology and Pedagogy of Reading," pp. 174 f.

part at least, to an achievement of a certain amount of visual reading. If we may trust A's introspections he handled the double process to a certain extent by getting the meaning of what he read visually and thus releasing the articulatory process for control of the writing. Pintner<sup>12</sup> has shown that, with practise, reading without articulation may be acquired when the articulation of what is read is artificially inhibited. He reports, further, "That practise in reading without articulation tends to aid ordinary reading, most probably by shortening the habitual visual reading." In the same way A carried over into the normal reading habits acquired during the reading from distraction.

As was said before no effort was made to test the memory of what was read but both reagents followed the story with interest.

The rate for reading aloud was obtained on sixteen different days for D. This ranged from 3.83 to 5.65 words per second. For A we have thirty normals obtained on as many different days. His rate ranges from 3.3 to 5.15 words per second. Such reading rates are excellent. To quote Huey again,<sup>13</sup> "In reading aloud, the average of the slowest reader was 2.2 words per second and that of the fastest 4.7, at the ordinary rate, and at maximal speed the corresponding range was from 2.9 to 6.4 per second."

The tendency to progressive increase in the rate of reading aloud is perfectly evident although, as before, interest in contents and familiarity with an author's style affected speed. D's record, given in connection with the dictation experiment (Table VII), shows that between the eighth and ninth records a break of more than two weeks occurred. Following this break there is a series of low records. These low records may be due to the break in practise but, more likely, are to be attributed to the bad physical condition of D at the time. With improvement in condition, May 30, there came rapid improvement.

The increase in the normal rate of reading aloud was due very largely to improved methods of breathing. Both reagents lowered their voices in the later experiments and there was some tendency to blur enunciation. D's enunciation is more distinct than A's. Relatively her reading aloud is much more rapid than her silent reading. It is difficult to explain this

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<sup>12</sup> "Inner Speech during Silent Reading," *Psychol. Rev.* XX, 1913, pp. 129-153.

<sup>13</sup> *Op. cit.*, p. 175.

unless the sound of her own voice acts as a spur to attention. This may well be the case as D is unusually sensitive to auditory stimulation. A, on the other hand, pays no attention to the sound of his own voice and finds that reading aloud disturbs the tendency to visual reading. A found at times a tendency to glance ahead and get the meaning before resorting to the slow reading aloud.

D was able to verbalize mentally the test-verse more rapidly than could A. Possibly the fact that D's inner speech is auditory-vocal-motor and that A's is vocal-motor accounts for this. The range for A (14 trials, different days), is from 3 to 6 seconds. For D (6 trials), from 2.5 to 3 seconds.

It is obvious that the progressive increase in the speed of the normal operations introduces some difficulty in interpreting the results under distraction, for a record under distraction that approximates the normal for the day may yet be far from the normal that might be reached under sufficiently high concentration of attention. Only if a fairly constant normal were attained, one obviously approximating a physiological limit, would we have at hand a satisfactory standard for checking the rate under distraction. In the time at our disposal it was, however, impossible to carry the experiment out to such an extreme. A's writing record on the first verse (33 seconds) may, however, be taken as a satisfactory limiting value. The final normals for reading aloud must also be very close to the physiological limit for the readers in question as evidenced both by the effort involved in the spurting and by the tendency to slur words.

## 2. *Rapidity under Distraction*

In the course of the discussion we have already indicated the general bearing of distraction upon rapidity. In the first place we find with practise a progressive increase in the speed with which reading and writing (or reading and adding) could be carried on. All the tables could be converted into characteristic learning curves. A consideration of the bearing of this fact upon the general interpretation of results will be attempted later.

Let us now summarize the facts recorded relatively to the speed of work under distraction.

In the test on simultaneous writing and silent reading there were several instances where A's most rapid rate of writing for the day was within a second of his normal record, or, more correctly, since the normal was taken without a screen, within two seconds of the normal. There are cases where

the most rapid record is the same or lower than the normal and the average time only slightly (two to three seconds) above the normal. Toward the close of the tests, the mean variations on the time-records were very low. There were also some very high reading-rates. Thus, in reading Chapter VIII of "The Abysmal Brute" under distraction A reached his second highest speed record for that story, a very high speed, something over eleven words per second. No doubt A's ability to read visually served the purpose of releasing the vocal-motor process for control of the handwriting. The average writing speed for this day is within three seconds of the normal. Since A's writing-habits are exceedingly well-organized and smooth, the only thing needed for maintaining the writing of the verse was a schematic and rapid vocalization of a few words. To this we must add also a capacity for extending the scope of attention.

D's writing-speed under distraction approached the normal less closely than did A's. The best records are found in Table II. For "The Abysmal Brute" the best average is ten seconds higher than the normal, and the record speed under distraction at least 4.8 seconds higher than the normal. In "The Game," the best average is about three seconds above the normal but there are one or two individual records below the normal. Very great retardation is evident for the poorest records. On account of her slower reading D wrote the verse a much greater number of times than did A but in spite of this failed to reach a constant reaction as soon. Had the experiment been continued with her she would have reached, probably, a more constant reaction. Her writing-habits are, however, much less mechanized than A's and her span of attention narrower. Under distraction her reading-rate in no case approximated the normal.

Let us turn now to A's record for writing while reading aloud. Between this experiment and that of silent reading the adding test intervened so that more than a month's time separated the two reading tests. In order to make the two series of records comparable the verse written in the silent reading test was again employed. It is interesting to note that the normal time for writing this verse began in the later series well towards the lower limit of the earlier series. The retardation of the speed under distraction shows, however, that reading aloud while writing, is a very much more difficult thing for A than is simultaneous silent reading and writing, no doubt because of the inhibition of visual reading.

In part, however, at least toward the close of the series A still had recourse to visual reading since he would glance back to take in the meaning, then resort to automatic vocalization of the words read while concentrating for a short period on mental vocalization of the verse. The initial record in this series is much higher than that in the first series. The experiment ran nine days before A reached an average writing speed as rapid as that reached on the first trial with silent reading. Toward the close of the series he made, it is true, an even better time-record under distraction than in the preceding series since once his verse-time fell to 34.5 seconds as against 39 seconds in the other. During the course of the experiment the normal time had, however, been constantly falling, so that the best record under distraction is 1.4 seconds above that day's normal record, while the best averages are from three to four seconds above the normal. A much greater variation in speed than occurred in the preceding series is also evident. The worst records show considerable speed retardation. A's reading-rate during writing is in this test considerably retarded, relatively much more so than the writing speed. There is, even in the fastest records, a loss of about a word per second. It would seem that the conditions of the test introduced a certain amount of vocal-motor conflict. The inner verbalization of the verse required from three to six seconds for A, hence a retardation of reading-speed from twelve to twenty-four words every verse-time (something over thirty seconds) would serve to keep the processes going. The retardation of reading speed is just about what would be required. Throughout, A's memory of what was read was excellent.

In simultaneous writing and adding A's writing-speed is retarded six seconds at least, and the adding speed at least three seconds. It is interesting that again the retardation in adding should be just about that required for thinking the verse.

In the experiment on taking dictation while reading aloud neither reagent approximated the normal reading-rate and that too in spite of the fact that both reagents felt the task to be a fairly simple one and made every effort to speed the reading.

### 3. *Graphic Lapses*

A careful tabulation was made of the graphic lapses occurring during simultaneous writing and reading and much material has been collected for subsequent study. At present

we shall confine ourselves to summarizing the general observations.

D's lapses during silent reading and writing were surprisingly few in number but significant in character. Often verses were written without a single lapse. At most, if we omit line-lapses, four lapses per verse occurred. The lapses that appeared were stereotyped. Such a stereotyped lapse occurred in the doubling of "v" and the repetition of the first letter-stroke of "y." The kinaesthetic report for these letters was absolutely lacking. Even after D was informed of such a lapse and attempted to inhibit it, it kept reappearing. This loss of kinaesthetic report on certain letters is curious. At times D reported, incorrectly, that she had dropped lines. Such omission actually occurred on occasion. Quite the most significant lapse that was noticed throughout the whole experiment for either reagent was the omission of a line in one verse with its repetition in the verse immediately successive (some sixty seconds later) without D's knowledge of either lapse.

A gave a great number of lapses for every verse. In the experiment on writing while reading silently these lapses consisted for the most part of lapses of omission such as the cutting of strokes, the omission of letter-parts or of letters and words, lapses similar to those which occur in his normal rapid writing, probably indicative of anticipations on the part of attention which keeps ahead of the writing. Line-lapses of omission or repetition were usually reported by A at the close of the verse. The number of omissions of letter-parts and of letters gradually increased throughout this series as a whole. Many omissions became stereotyped and a given word presented a progressive cutting of strokes. Thus "February" appears successively as "Febrary," "Febray," "Febry" and "Febu." Toward the close of this series of experiments repetitious lapses and lapses of substitution began to appear. There occurred also a curious exchange of letter-parts, a lapse purely motor in origin.

With the shift from silent reading to reading aloud a most interesting change occurred in the kind of lapses that were most frequent. The repetitious lapse appeared again and again. This lapse evidences the attentional conflicts and, perhaps, also some difficulty with the kinaesthetic report. An exchange of letters as well as of letter-parts appeared; thus, "April" became "Aprli." Hybrids, due to fusion of two words are found; thus, "gives twenty" becomes "gventy." The repetitious lapses showed a tendency to become stereotyped as had the omission-lapses in the preceding series.

On the fifteenth day of this experiment the repetitious lapses began to drop out noticeably and the old lapses of cutting strokes and omission of letters reappeared. Such a shift in kind of lapse evidences, as did the introspective report and the increased rapidity of writing, the growing skill with which attention handled the double process.

The shift from repetitious lapses to lapses of omission in this second series is more easily understood than the tendency in the simultaneous writing and silent reading to shift toward the close of the series from lapses of omission to repetitious lapses. Possibly in this later case the distraction has become so complete and the verbalization of the verse so cut that attention to the writing came only in jerks and at very infrequent intervals. The experimenter found that she could produce these lapses of repetition almost at will (without A's knowledge of the occurrence) by the simple device of moving the paper gently or of lightly touching A's hand. Such artificial production of lapses promises much in the way of future understanding of them.

Throughout the course of the experiment A was able by thinking back over the writing to recall many of the lapses made with surprising accuracy. There were times, however, when he reported a strong feeling of having made lapses which were not found to occur.

#### 4. *Simultaneity of Processes*

We are ready now to raise the question as to the actual *simultaneity* of mental processes during the course of the experiment. Simultaneity of motor reactions occurred. A was able to continue writing, even to start a new word while announcing the results of mental addition, and able to write while reading aloud. It was found, however, that even with the immense amount of practice reported the verse could not be written absolutely mechanically, simply by an unwinding of the movements; some slight control in mental terms was necessary. The mental verbalization of the first verse employed took from three to six seconds—that much retardation of reading-speed would probably serve the purposes of control. We have already indicated that just about so much retardation occurred in the reading-rate under distraction. There is, possibly, one exception, the rate of silent reading of Chap. VIII of "The Abysmal Brute" where A achieved an extraordinarily high reading speed with very little retardation of the writing speed. Here we may have a bit of evidence for actual simultaneity of the mental processes.

Introspectively, we get reports of actual synchronization.

A reported that while he was reading aloud an imaginal vocal-motor cue floated over the overt vocalization of the words read. It is obvious, however, that such a report must be checked by objective records. Still it is possible that the slight retardation in speed noticeable throughout the tests is a general retardation due to the amount of effort needed for keeping the two processes going rather than a specific retardation of either of the operations. The normals were taken with great effort of spurting and the attempt to drive two processes to the limit was very fatiguing.

Two interpretations of our records are, therefore, possible. The first is that two operations may be carried simultaneously with slight loss of speed if one be so mechanized as to require little sensory control; the other is that two mental operations may for a short period be actually synchronized but with a slight loss in efficiency due to the demands upon the general mental (nervous) energy.

The question of speed under distraction is of interest not merely as a check upon the subjective impressions of the reagents but also in connection with certain reports that have been made upon automatic writing. Thus Solomons and Stein write in their report upon "Normal Motor Automatism:" "In all automatism the tendency toward increased speed is marked. Writing tends towards a pace that very quickly tires, reading towards a rapidity that prevents distinct articulation, dictating towards a speed that soon becomes hopelessly fast for the writer."<sup>14</sup> In the above report no records are given of either reading or writing speed so that it is impossible to tell the precise data upon which such a conclusion is based. In our experiments we failed to attain, except in rare instances, an approximation of the normal possible speed. This failure was particularly true in the case of the dictation experiment which resembled rather closely the kind of experiment carried on by the writers referred to above. It is quite true that, subjectively, we had often a feeling of "furious" speed and in the verse-writing tests there are samples of writing showing, at least in the case of A, excessive and flourished movements, but, on the whole, there is evident speed-retardation of some sort.<sup>15</sup>

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<sup>14</sup> Psychol. Review, III, 1896, p. 507.

<sup>15</sup> Probably a greater rapidity than that of normal writing without speeding does result for certain individuals under distraction. The excitement of the distraction serves to introduce considerable tension; each operation serves as a spurt to the other. But the limits of the NORMAL SPURTED SPEED were not reached, in our experiments at least.



5. *Lapses of Awareness*

Again, in the matter of lapses of awareness our reports are in certain instances different from those of Solomons and Stein who distinguish two forms of lapse, lapse of memory and "real unconsciousness." They write,<sup>16</sup> "The consciousness without memory seems to *approach as its limit*, simply a condition in which the subject has not the faintest inkling of what he has written, but feels quite sure that he has been writing. It shows no tendency to pass beyond this into real unconsciousness. It seems to depend on the lack of associations between different words. And conversely real unconsciousness appears not as a final stage of a gradually decreasing memory, but quite suddenly. It does not seem to depend upon association elements at all."

We cannot affirm any distinction between "real unconsciousness" and "consciousness without memory." We found that sensory control could be reduced to a minimum both in writing a memorized verse and in taking dictation. Moreover, there were instances where the reagents reported a lapse of awareness covering perhaps several words but they were never confident that at the time of writing there had been no consciousness. As the experiment progressed we came to realize more and more the extent to which a reported lapse of awareness might be a lapse of memory instead. Throughout, there was evidence of progressive restriction of memory, failures to make associative connections, dependent very obviously upon practise. Thus in the dictation experiment breaks occurred, first, in sentence connections, then in phrasal connections,—each word became detached. At the close of even a short interval the reagents were able to recall little of what they had written although confident that at the time of writing they had been aware of it. In the case of D the breaks in connection went further. Meaning evaporated from detached words and sometimes in the middle of a word she forgot what it was.

The progressive failures in memory as the reagents learned with practise to concentrate upon reading; the fact that just about as much could be recalled at the close of a one as at the close of a three minute writing interval in the dictation experiment; the gradual loss of meaning, no doubt through breaks in connection; the more evident failures of memory when the reagents were in bad physical condition, led us to

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<sup>16</sup> *Op. cit.*, pp. 501 f.

believe that all lapses reported were memory lapses which approached as a limit a completely dissociated mental bit.

It is significant that A, whose span of attention is broader than D's, should have reported less thoroughgoing breaks than did D, and that he should have remembered so much more of the written contents than D, and at the same time have had also a relatively better memory of what had been read. His success in writing the verses while carrying on other operations seemed also dependent upon his ability to remember "where he was at," it was only toward the close that he began to complain that meaning was evaporating from the verse so that if he lost his place he was unable to continue with the verse unless he thought back over what he had written. D made this complaint much earlier.

In the above discussion we are thinking chiefly of the verbal (cue) consciousness. Movement itself may, it would seem, be a purely physiological occurrence but there is no reason why kinaesthetic sensation from hand and arm may not become dissociated as well as any form of verbal imagery. We have said before that A's writing-habits are so thoroughly mechanized and his writing-impulse so smooth that normally the return report from hand and arm does not enter the focus of attention; it is marginal. D, on the other hand, is acutely conscious of the movements of the hand and arm in writing and reported under distraction a drop in kinaesthetic awareness of the hand that was very distressing. With this went a curious loss of spatial orientation, with, on occasion, some strange illusions as to hand-movement. She found it impossible to maintain alignment, which in the successive writing of the verse became progressively more degenerate.

It would seem that neither of the reagents who served in the experiment was of the narrow span of attention that would seem to favor breaks in associative connections and, hence, loss of conscious integration. A's span of attention is probably exceptional. With subjects of a different attention-type more frequent reports of lapse of awareness might have resulted. In any case material is accumulating as to the probability of striking individual differences in the ease with which consciousness may be "restricted" so to speak. The habit of fainting, the varying susceptibility to intoxication, and of amenability to anaesthetics, the varying ease with which individuals fall asleep point to striking differences in the facility with which breaks in connection occur. It was our impression from the whole course of the experiment that a report of unawareness was certainly conditioned by lack of

memory-connections and that, possibly, consciousness itself should be phrased wholly in terms of connection.<sup>17</sup>

We may meet at this point a few criticisms which may be urged against our method of attempting to get samples of automatic writing. It may be said that the verse tests were such as to demand conscious control, at least to some degree; or that, on the other hand, the use of a memorized content might have led to an illusion of a complete memory since, after the writing was completed, one knew at least what he was supposed to have written. Moreover, the calling for introspective reports on the course of the experiment may have developed an intention to remember and such an intention is without doubt very influential in determining recall. Asking for a report by the reagent of the words written in the dictation experiment may for the same reason be cited as an error in method. But testing of the memory was, of course, necessary in some form. It may be said that our dictation tests were run for too short a period to permit absorption in the story. We had, however, so trained ourselves that we plunged into the reading without effort. Moreover, in the supplementary test we ran the writing for long periods without obtaining any essentially different results. Except with short periods of work it would have been impossible to check the memory reports for, under the conditions of the experiment, memory of what has been done vanishes with very great rapidity. It is the failure to offer any evidence of the checking of general introspective reports that one feels to be a criticism upon the valuable report of Solomons and Stein.

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<sup>17</sup> In certain respects this conclusion as to the character of automatic writing may seem at variance with that reached in my monograph on "Control Processes in Modified Handwriting" (Psychol. Rev. Monograph Series, 37). In that report emphasis was placed upon the kind of control utilized in writing under distraction; here, the emphasis is on the functioning of attention; in the former report the initial response to different sorts of distraction was studied; in this report we have worked on the effect of practise on a double operation. The two investigations are supplementary. I have, however, been impressed during the course of the present investigation by the extent to which certain reports of unawareness are certainly conditioned by failure to make connections and by the value of a limiting conception of a detached bit of consciousness. Certainly, in some curious way, the kinaesthetic side represents the crucial point in the situation. Thus D would seem to offer better evidence for the psychophysiological interpretation of automatism than A. The most important difference in the reactions of these reagents concerns the nature and co-ordination of the motor impulse, with, in addition, some evident difference in the natural scope of attention. (J. E. Downey.)

# THE STUDY OF DREAMS<sup>1</sup>

## A METHOD ADAPTED TO THE SEMINARY

By MADISON BENTLEY, University of Illinois

The clinical study of dreams has, in our times, attracted a great deal of attention. By many students of the abnormal it is held to be a valuable aid in the diagnosis of mental disorders. The dream, as interpreted by the clinician, is a residue of early emotions and of old desires which find, in sleep, a deferred and vicarious satisfaction. Whatever the diagnostic import of the dream may be, however, it is clear that the psychologist cannot derive from the clinician's methods of divination a coherent descriptive account of the dreaming mind. If dreams are symptoms and symbols, they are also facts of consciousness, facts which present characteristic constellations, which take their proper courses, and which carry functions different in degree, at least, from those of the normal consciousness of the daytime. This clinical method, then, is not well adapted to the study of the dream considered on its own account.

Within a few years, experiment has notably enlarged our knowledge of the dream, particularly of the immediate dependence of the dream upon stimulus and upon bodily state. The studies of de Sanctis,<sup>2</sup> Vold,<sup>3</sup> Weygandt,<sup>4</sup> and others have made it evident, in a striking manner, that motive and incident may be supplied to the dream in a great variety of ways through bodily interference. The application of heat and cold, of lights, sounds, odors, pressures, the constraint and disposition of bodily members, and many other means to stimulation of sense organs, may—as we now know—determine or modify both the main contents and the obscurer context of the dream. Such studies as these afford admirable control of the psycho-

<sup>1</sup> Studies in Psychology from the University of Illinois.

<sup>2</sup> de Sanctis, S., *Die Träume*, Halle, 1901; — and Neyroz, U., *Psych. Rev.*, ix, 1902, 254.

<sup>3</sup> Vold, J. M., III. *Internat. Congress f. Psychol.* (1896), 1897; *Ueber den Traum*, Leipzig, i, 1910, ii, 1912.

<sup>4</sup> Weygandt, W., *Entstehung der Träume*, (Diss.), Leipzig, 1893; *Philos. Stud.*, xx, 1902, 456; *Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg.*, xxxix, 1905, 1.

physical conditions which obtain during sleep. They have, moreover, led to extremely clever modifications of empirical technique. It should be written down to their credit, too, that these experimental essays set a useful check upon the looser divining methods of interpretation, methods which run the risk of personal bias and of doctrinal contamination.<sup>5</sup>

But neither has the method of abrupt initiation and occasional interference given—any more than the diagnostic method—a delineative account of the dream. To this end careful and complete introspection, taken under the best conditions, seems to be the most direct and the most natural means. Various objections,—it is true,—to this direct method have been urged; the impossibility of immediate report, the abnormal state of the sleeper, subsequent amnesia, and the like. The serious attempt to make methodical record of one's own dreams, for psychological ends, suggests, however, that the objections offered to the method strike at difficulties which are not really insuperable.<sup>6</sup>

But not every dreamer's tale is an introspection. Those recitals of the dream which are made for dramatic purposes, or for entertainment, or as a cathartic means of relieving the oppressed mind, have, it must be admitted, no scientific use; and the dream-story, retailed by the psychologist himself, has,

<sup>5</sup> J. J. Putnam, a believer in the clinical analysis of dreams, speaks of "alleged eccentricities (doubtless sometimes real) in interpretation." *Jour. of Abn. Psych.*, ix, 1914, 36.

<sup>6</sup> Among recent applications of the introspective method to the investigation of dreams should be mentioned the studies of A. Aall (*Zeitschr. f. Psychol.*, lxx, 1914, 125), F. Hacker (*Arch. f. d. ges. Psychol.*, xxi, 1911, 1) and P. Köhler (*ibid.*, xxiii, 1912, 415; xxvii, 1913, 235). Aall prescribes as the proper method of observation (*subjektive Reflexionsmethode*) the careful, immediate and unbiased reproduction of the dream by a trained observer, in a quiet dream-like state, with a critical survey of the antecedent waking conditions (p. 126). Hacker, who observed systematically for nearly a year and a half, lays emphasis upon the necessity for (1) immediate transcription and (2) exact and searching analysis and description of the dream-consciousness (pp. 2ff). At the suggestion of Professor Külpe, H. made a special study of thought in dreams. Köhler's observations were based upon about 600 dream-records. He profited by the fact that he is accustomed to waken "twice every night,—rarely three times," by setting down the records during the waking periods of ½-1½ hours. Both K. and H. give curves for depth-of-sleep (Michelson's auditory method). These three studies mark a notable advancement in method over older studies. Our own work differs from them chiefly in its subdivision of the task and in the more elaborate fractionation of the introspections.—The possibilities of combining clinical and analytical motives in the study of the dream are illustrated by the article of E. R. Thompson (*Brit. Jour. of Psych.*, vii, 1914, 300), which was inspired by Freud's *Die Traumdeutung*.

if it is taken as it stands, only a qualified value in description. The recital, though it be given accurately and faithfully, is no more and no less introspective than the account of a ball-game or an enumeration of observed paintings in a gallery. It differs from these only in the extent to which the events detailed are controlled by external or by organic conditions. Like them, it furnishes only gross "information," the raw material from which introspective descriptions have carefully to be separated.

In part, the allurements of the dream-consciousness itself, and in part the possibilities it offers for introspective training led us to choose (in the spring of 1914) the subject of dreams as a topic for the seminary. After historical summaries of the literature, from Aristotle to Freud, had been brought in and discussed, as many members as found the task feasible began the nightly quest for dream materials.

The conditions of the quest were made as follows. In the hand of each observer was placed a copy of the outline reproduced upon the opposite page (Table I). Simplified copies (one for the record of each dream), which contained spaces to be filled in by the observer, were also distributed before the work began. At the same time the Obs. was supplied with candles, matches, pencil, blank paper, an alarm clock, and a sealed schedule of hours of waking made out for the following fortnight. Upon retiring at night the alarm was set, without O's knowledge, according to the schedule; and the necessary materials were placed within reach of his hand. The schedule of hours was so chosen, by a person not an observer, that the waking-signals should ring as if by chance with an equal distribution throughout the period of sleep. Only one arousal was arranged for in any single night. Upon a given evening, then, O knew only that the alarm was to sound at some time before his normal hour for waking. The length and violence of the signal were adjusted by a preliminary practice to the needs of the individual. Thus one Obs. would require only a muffled tap of the bell, while another awakened at the end of a long din released close to his ear. The Obs. was cautioned to begin his record only after he had accustomed himself to a quiet arousal at the alarm. He was instructed to take the introspective attitude (without moving and with eyes still closed)<sup>7</sup> and to wait passively for the resurgence or the *Abklingen* of the dream.<sup>8</sup>

If no dream was at first used, the Obs. recorded the fact, together with the hour, and returned to his slumbers. If a dream appeared, he

<sup>7</sup> P. Köhler, (*Arch. f. ges. Psychol.*, xxiii, 1912, 417) also noticed that the dream tends to disappear under bodily movement.

<sup>8</sup> In 1893 M. W. Calkins made a statistical study of nearly 400 dreams of two Obs. (*Amer. Jour. of Psych.*, v, 1893, 311). In this study the alarm clock was at first used, but soon discarded, the Obs. trusting to accident to rouse him from sleep after dreaming. The method seems to have been suggested to Professor Calkins by J. Nelson (*ibid.*, i, 1888, 381ff.). It is traceable to L. F. A. Maury (*Le sommeil et les rêves*, Paris, 4th ed., 1878, 2ff.), who engaged a person to arouse him during the night. For a full discussion of the method see Foucault, M., *Le rêve*, Paris, 1906, 9ff.

was to trace its main features, then to light his candle, set the dream down photographically (sect. I, 1, in Table I), and fill in under the other rubrics as much as could not safely be left to the next day. Upon waking in the morning, the rest of the record was filled out. At our meetings, held fortnightly, the individual records were brought in, together with a summarized report made out upon a blank form which was supplied for the purpose.<sup>9</sup>

TABLE I

Obs.....	Date.....	Time	<div> <div>Bed.....</div> <div>Asleep.....</div> <div>Aroused.....</div> </div>
General psychophysical condition		Remarks	
I.	1) Recital:	D. events given in order	
II.	2) Description:	translation into process-terms (including simple feelings and attitudes)	
	3) Temporal continuity and sequence (or absence)		
	4) Associative formations	<div> <div>Assimilation</div> <div>Free images</div> <div>Discursive A</div> <div>Topical A</div> </div>	
	5) Configuration	<div> <div>Foreground, background</div> <div>Broad, narrow, deep, shallow</div> <div>Degrees of clearness</div> <div>Sustained or fluctuating attention</div> <div>Primary, secondary or derived-primary attention</div> </div>	
	6) Mental functions	<div> <div>Perception, ideation, memory, imagination</div> <div>Volition, emotion, sentiment, recognition</div> <div>Thought</div> <div>(or apprehensive, executive and elaborative)</div> </div>	
III.	7) Waking residue of D.	<div> <div>Mental</div> <div>Bodily</div> </div>	
IV.	8) Persons	<div> <div>Known</div> <div>Unknown</div> </div>	<div> <div>And Scenes</div> <div>Recent</div> <div>Remote</div> </div>
V.	9) Relevant bodily and external events during sleep (e.g., pain, cramp, muscular tension, fatigue, light, cold, sound)		
VI.	10) Reference to past events: waking events, concerns, interests, fears and desires	<div> <div>Evening before</div> <div>Day before</div> <div>Earlier</div> </div>	
	11) Reference to character, temperament, ideals, principles, individual traits		
VII.	12) Gross comparison with waking consciousness	<div> <div>Likenesses</div> <div>Differences</div> </div>	
	(e.g., processes, coherence, rate, attention, shift of function, types of association)		

<sup>9</sup> The department is prepared to furnish mimeographed copies of the outline and of the blanks to psychologists who will undertake to collect additional material.

It was not to be expected that each dream would furnish exhaustive materials under each of the twelve headings; instead we hoped to supply, by a kind of fractionation, from one record what was wanting to another. Our chief modifications of the method were (1) the equal and predetermined distribution of arousals throughout the night, (2) the separation of the dream-recital from the introspective description, (3) analysis, by headings, of the introspections, and (4) the giving of instructions which should define and limit the task or *Aufgabe*.<sup>10</sup>

### Results.

I. *The Dream and the Sleeping Period.* Since the alarm-signals were distributed with approximate regularity throughout the night, it is possible to make out the relation of dreams to the course of the sleeping period. The signals for waking were arranged to fall on the half-hours, the earliest at 11:30 and the latest at 5:30. The limits were so chosen as to fall within the borders of the customary sleeping period of the observers.<sup>11</sup> If dreams were equally frequent throughout the night and if circumstances for their waking revival were the same from hour to hour, then we should expect to find a like number of dream-records upon each of the signal-hours. Experiments made upon the depth of sleep<sup>12</sup> and dream-records previously published<sup>13</sup> alike lead us, however, to anticipate an unequal distribution. The following Table (II) indicates that most dreams appeared, for our observers, after the second hour of sleep was finished. Only one dream is recorded earlier than two and three-quarters hours after the initiation of sleep. This dream comes from the writer's record and it was noted as a "mere ghost," almost entirely wanting in describable contents. The totals for the whole group of five persons, running from hour to hour, stand as follows:

<sup>10</sup> The essential difference obtaining between the clinical method and the method here proposed will be made evident by a comparison of our Table I with the "dream analysis records" recently published by L. H. Horton (*Jour. of Abn. Psych.*, viii, 1914, 393).

<sup>11</sup> It occasionally happened, of course, that the Obs. was awake at the time of the alarm; but such exceptions were noted.

<sup>12</sup> Michelson, E., (Diss.) Dorpat, 1891, and *Psych. Arbeiten*, ii, 1899, 84 (Michelson cites the work of Kohlschütter, and of Mönninghoff and Piesbergen); Herrwagen, F., *Philos. Stud.*, v, 1889, 301; Sanctis, S. de, and Neyroz, U., *op. cit.*

<sup>13</sup> Calkins, M. W., *op. cit.*; Weed, S. C., and Hallam, F. M., *Amer. Jour. of Psych.*, vii, 1895, 405; Monroe, W. S., *Jour. of Philos. Psych. and S. M.*, ii, 1905, 650; Spiller, G., *The Mind of Man*, 1902, 430ff.



TABLE II

Hour of Sleep	0-1.....	0
" " "	1-2.....	1
" " "	2-3.....	1
" " "	3-4.....	4
" " "	4-5.....	12
" " "	5-6.....	9
" " "	6-7.....	12
" " "	7-8.....	5
Total <sup>14</sup> .....		44

As regards the small number of dreams recorded for the early hours, there are three possible explanations: (1) The early alarm failed to waken the sleeper, (2) the condition of the Obs. was unfavorable to the discovery of dreams (*e. g.*, on account of the great depth of sleep), and (3) early dreams were actually infrequent. Let us examine the possibilities. Obs. C was aroused only once from 19 alarm signals falling within hours 0-3, and only three times from 25 alarm-signals within hours 3-5. No one of these arousals gave a dream, whereas the 11 arousals in hours 5-7 gave five dreams. Out of 84 signals (Feb. 21-May 22), Obs. A failed to hear only 9, and of these 3, or, possibly 4, fell within the hours 0-3. This Obs. reported a single dream within the first three hours of sleep, out of 13 dreams in all. The other three Obs., for whom data are not quite complete, also returned a number of early dreams disproportionally small for the frequency of arousal. The result accords with unpublished records of the writer, in which a long period of alarm-clock dreams (above 40) gave only blanks or traces for the first two hours of sleep. As regards the second possibility,—loss of early dreams by conditions unfavorable to their recovery,—the records do undoubtedly indicate that the sleeper is apt to be especially stupid and soporose when awakened early in the night. In spite of this fact, however, we were, even while somnolent, clearly aware of the task and we were likewise able to follow the instructions carried over from the evening before. It seems fairly evident, then, that although the early hours of sleep are unfavorable both to arousal and to the introspective attitude, these unfavorable conditions do not fully account for the want of dreams at this time. The absence of dreams is presumably to be referred to the extreme modification of bodily functions incident to deep sleep. It is still arguable, of course, that this extreme modification destroys the incentives to reproduction

<sup>14</sup> Each arousal with a dream is counted as one, although two or more dreams were sometimes reported at the same time.

and produces complete amnesia. The presence of the definite *Aufgabe* to recall makes improbable this assumption, however, which seems, in spite of its frequent support in the literature, to stand in want of positive evidence.<sup>15</sup>

II. *Contents, Course, Configuration and Functions of the Dream* (Sect. II in Table I). We may begin with the processes which make up our 54 dreams. Table III gives the introspective analysis in terms of sensory and affective contents and of attitudes.

TABLE III<sup>16</sup>

Obs.	No. D's.	Visual		Aud'y		Aud'y-Kin.		Kin.	Tact.	Aff'n		Attitude
		Color	Grey	Repr.	Verb.	Repr.	Verb.			Pl.	Unpl.	
A	15	7	15	1	14	..	1	4	3	7	11	16
B	14	1	16	4	14	..	..	6	2	3	8	4
C	9	1	6	..	5	..	..	4	..	1	5	5
D	4	..	4	2	2	..	1	4	..	3	1	7
E	12	2	7	2	7	2	..	1	..	..	2	5
	54	11	48	9	42	2	2	19	5	14	27	37

The frequency of occurrence of sensory and imaginal processes is not especially significant. Our number is too small for statistical treatment.<sup>17</sup> Visual(59) and auditory(51) processes play the chief rôle. Greys(48) are more frequent than colors(11); and verbal auditory(42) processes than tones and noises which are immediately representative of sounding

<sup>15</sup> We leave entirely out of account the period immediately following the initiation of sleep. This period is to be covered by an especial inquiry which will combine experimental control of the hypnagogic state with the present method of arousal and report. Hints at a method are to be found in Ladd, G. T., *Mind*, N. S. i, 1892, 299; Müller, J., *Ueber die phantastischen Gesichterscheinerungen*, 1826, 49; Weygandt, W. E., *Philos. Stud.*, xx, 1902, 464; Vold, J. M., *III. international. Cong., etc.*, 355; Spiller, G., *op. cit.*, 430.

<sup>16</sup> "Repr" = representative or primary image; "verb" = verbal image; "kin" = kinaesthetic.

<sup>17</sup> Nevertheless the relative frequency of processes from the several sense-modalities stands in substantial agreement with the results of Calkins, M. W., *op. cit.*, 321ff; Weed, S. C., and Hallam, F. M., *op. cit.*, 405ff; Monroe, W. S., *op. cit.*, 650; Hacker, F., *op. cit.*, 16; and Köhler, P., *op. cit.* (1912), 433ff. Thompson, E. R. (*op. cit.*, 305) found among his five subjects a like distribution of imageries in the waking and dreaming consciousnesses. Cf. Beaunis, H., *Amer. Jour. of Psych.*, xiv, 1903, 271.

objects(9). For the most part, the visual materials furnish the scenery of the dream; they provide the perceptual panorama; while verbal processes, in auditory form, carry conversations and give descriptive filling to the events. The infrequency of auditory-kinaesthetic fusions(4) is noticeable. The kinaesthetic elements common to verbal formations in waking life seem to drop out of the dream-words. Kinaesthetic processes, such as were involved in running, opening a door, rubbing objects in the hand, were reported occasionally by all dreamers(19), and in the cases of A and B tactual elements were added. Affection was recorded in 41 cases, unpleasantness about twice as often as pleasantness. The conscious attitudes (*Bewusstseinslagen*) form an important factor in the dream. Most of them bear the aspect of condensed and fore-shortened emotions. They are surprise(7), futile worry(5), familiarity(5), hurry(4), perplexity(3), searching anxiety(2), incongruity, foreboding, expectancy, bewilderment, doubt, apology, distaste, dislike, responsibility, confusion and relief,—in all, 37. It is not unlikely that in waking hours many of these attitudes would have been obviously complex, representing emotive adjustments or moods; but as they stood in the dream they appeared to be elemental. It seems likely therefore that this emotive condensation is especially frequent in the dream-state. In a number of cases, however, the attitude is undoubtedly engendered not by the dream-event, but by the instructions. Thus perplexity, futile worry, and searching anxiety were directly traceable,—at least in some dreams,—to the task of reporting. If they were translated into verbal processes they would appear in such forms as “Does my dream really take this course?” “Can I not recover the true ending of this event?” “Have I recovered all the processes that actually ran their course?” The attitude seems to form a part of the tissue of the dream itself, and not to be introduced from the subsequent period of introspection. This sort of attitude is, as we all know, not limited to the dream. Every introspective psychologist recognizes the attitudes of—“responsibility,” may we not call them?—which derive from, and, in part, *are*, the instructions lying behind or underneath the observation. In fact, every scientific observation which has not become automatized by practice or habit, involves, I suppose, either the “responsibility” attitude or its expanded and elaborated surrogates.

*The course* of the dream is commonly described as broken, disjointed, and interrupted; as lacking the continuity imported

into our waking life by perceptual and ideational trains and by sustained interests, occupations, and the like. We found, however, different sorts of dreams, among which we were able to distinguish four types as regards temporal course; fragmentary(11), disjunctive(13) (*i. e.*, abrupt changes and turns, without, however, a loss of integrity), and continuous(19) dreams, and finally, successive dreams carrying a common topic(6).

The account of the *associative formations* in the dream is necessarily gross. We sought only to identify the general types of constellation and sequence. Groups of images were, as might have been expected, most frequent. They carry, in large measure, the scenes and the persons of the dream. Psychologists who hold strictly to the peripheral origin of dreams obviously overlook the fact that while a dream may be initiated or turned by external or bodily events and agencies these events and agencies furnish no adequate explanation of the main part of the dream's contents. To say, for example, that a flash of light turned upon the sleeper's eyelids or a breath of cold air blown across his face produces a dream of celestial or of arctic regions is only to mean that the photic or thermal stimulus releases a set of central functions which themselves incorporate the dream.<sup>18</sup> The traditional account of the dream makes it discursive: it is said to pass abruptly from topic to topic and from scene to irrelevant scene. However, our incomplete survey of associative type reveals both discursive dreams(17) and almost as many(13) topical dreams. The term "topical"<sup>19</sup> does not indeed imply that dream-topics

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<sup>18</sup> Even in the hypnagogic or initial dream where the organism is still, to some extent, *en rapport* with environmental events it is easy to over-estimate the part played by stimulus. Cf. Ladd's clever explanation of early visual dreams in terms of retinal figures (*op. cit.*). The peripheral or sensory origin of dreams is frequently urged in the literature. The only empirical basis for the theory seems to rest upon the observation that *some* dreams are touched off or turned in their course by stimulus. Hacker (*op. cit.*, 97) credits Purkinje,—as I think wrongly,—with a sensational theory of dreams. Purkinje distinguished sensory, associative, and "complete" dreams. In the last-named "sind alle Seelenvermögen . . . repräsentirt." "Wenn auch der Traum sein sinnliches Material nicht anders woher, als aus dem wachen Leben nehmen kann, so leidet doch dadurch die Freiheit des innern Subjects mit allen seinen geistigen Anlagen keine wesentliche Beschränkung." R. Wagner's *Handwörterbuch der Physiologie*, u. s. w., III.<sup>2</sup> 1846, 452, 456.

<sup>19</sup> See Ebbinghaus, H., *Grundzüge d. Psychol.*, 2nd ed., i, 1905, 694.

were effectively or even sanely treated; but rather that the associative lines ran from persistent topical centres.

The following Table (IV) gives our results for *Configuration and State*. The first three headings indicate the degree of clearness of the dream taken as a whole; the second three the presence of foreground only, of background only, or of both. Wanting a like description of similar total consciousnesses in the waking state, these figures are not very significant. Moreover, the introspections are difficult and they need highly

TABLE IV

Clear	Unclear	Very obscure	Foreground	Background	Fore- and Background
25	9	5	8	15	23

trained observers, for there is danger of confusing memorial fading with primary obscurity. But anyone who is accustomed to introspective work upon attention is able properly to instruct himself for the dream-task. The results may be taken to indicate, at least, that the obscure processes play a greater part in the dream than in waking life. As regards the type of attention, our observers brought in 36 instances of primary and six of secondary or "voluntary" attention. The latter referred to problems of action and of thought.<sup>21</sup>

Wanting a classification or even a catalogue of *mental functions* to serve the purposes of current psychology it was not easy to formulate our instructions under this heading. The seminary contented itself with the use of the gross traditional terms, first coming to an understanding of its own interpretation of them. The topic needs a separate inquiry of its own. The distribution of functions in our reports follows:

TABLE V

Perception	Action	Emotion	Imagination	Aesthetic Sentiments	Thought
48	2	39	1	4	5

Hacker refers the rapid shifts and turns of the dream to the want of determining tendencies, and the bizarre combination of ideas to the fact that the processes tend, as they accumulate in a constellation, to annex subsequent processes which would have fallen, in the waking consciousness, into quite different integrations. On the other hand, Köhler believes that determining tendencies underlay a number of his dreams (*op. cit.*, 1912, 482).

<sup>21</sup> *e. g.*, Obs. A: "Secondary attention occurred when I was listening to the reading of a letter, when I was trying to perform an experiment with rats in spite of numerous distractions, and when I was taking part in an argument."

The term "perception" stands first. Were it not for the frequent confusion among psychologists of perceptual and memorial functions, it would hardly be necessary to explain that "perception" as here used refers neither to the actual presence of objects nor to the adjustment of the organism to its environment. It is a purely mental function and it stands for the apprehension of persons and things, without enquiring whether the persons and things are actually and really *present*. With such an epistemological question the perceiving consciousness has nothing to do. On the other hand, "memory" was taken to mean, not that the previous experience was found by inference to furnish dream materials, but that the dreaming consciousness made direct and explicit reference to a "past" which was distinguished, *within the dream*, from the perceptual or ideational or emotional "present." The term "imagination" is to be taken in the same strict sense. There were no authenticated cases of memory in our dreams,<sup>22</sup> and only one of imagination.<sup>23</sup>

The discrepancy between the number of dreams with perceptions(48) and the total number recorded(54) calls for explanation. To many the dream is just a perceptual panorama. We found several dreams, however, without "scenery" and without events. One was made up of an attitude of expectancy, another was anxious waiting, and a third was "a vague struggle to formulate some such thing as a rule." It seems likely that in dreams of this kind the only conscious processes carried in sleep are those processes which bear the instructions from the preceding day. Such shreds of consciousness may possibly be responsible for the predetermined wakening of persons who "set" themselves for early arousal.

Unlike the attitudes, the *dream-emotions* were seldom referable to instructions. They sprang instead from total situations or scenes, and they include embarrassment(7), fear(7), anger(6), resentment(3), wounded feelings, loneliness, vexation,

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<sup>22</sup> Calkins (*op. cit.*, p. 324) reports several cases of dream-memory. However, the context does not make it quite clear that the memorial reference was, in these cases, within the dream-consciousness. The same thing may be said of Köhler's "memories" and "imaginings" which he found in 1/5 of all his dreams. They seem to differ from other ideational processes, not in function, but in attributes; they are, as K. explains, less vivid and corporeal, more indeterminate and fleeting, and less definitely localized (*op. cit.*, 1912, 438).

<sup>23</sup> The only instance was reported by A: "A sort of living Greek frieze. I imagined it and did not really perceive it."

annoyance, mild joy, sorrow, indignation and remorse; nearly all of them unpleasantly toned.<sup>24</sup> Although *action* played a small part in the dream consciousness, as the recitals abundantly testify, it seemed probable to us, at the end, that we had at times overlooked obscure processes concerned with such automatized and habituated actions as walking, grasping objects, and changing bodily position. It remains true, however, that such a thing as an "action-consciousness," a constellation of clear and dark processes preceding and accompanying organic movement was, in our dreams, exceedingly rare. This fact may be connected directly or indirectly with the general motor quiescence of the body.

III. *External and Historical References of the Dream* III(7), IV(8), V(9), VI(10). The immediate residues of the dream were surprisingly scanty. Four times the observers noted a haunting effect left upon the subsequent waking consciousness and nine times bodily disturbances (such as trembling from fear) were referable backward to the dream-state. The psychophysical residues of dreams seem to be much more prominent in those impressive morning dreams which we carry over into the active affairs of the day. Of the many *persons* who appeared in our dreams, we recognized 64 in the recital, while 45 were unknown. Besides, there were frequently supernumerary figures who were massed in the background and observed neither as strangers nor as acquaintances. Of the *scenes*, 14 were familiar and 44 were unfamiliar. The unfamiliar, however, revealed at times glimpses of recognizable places. Of the dream items (*persons*, *places*, *scenes*, *events*, etc.), 37 were reminiscent of waking occurrences of the previous evening, 20 seemed to have been derived from the day preceding, and 24 from earlier waking experiences. It is probable that a stricter search would have shown recent revivals also of the earlier experiences. The obvious reference to an earlier event tends to make the observer disregard a more re-

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<sup>24</sup> This result agrees with Calkins (*op. cit.*, p. 327), whose list of emotions displays a remarkably close resemblance to ours. In 3 of the 4 observers of Weed and Hallam (*op. cit.*, p. 409), unpleasant dreams were much more frequent than pleasant. Weygandt (*Philos. Stud.*, xx, 1902, 488), refers the preponderance of unpleasantness in dreams partly to unfulfilled wishes and partly to such organic stimuli as hunger, thirst, and labored breathing. Most of our emotive unpleasantnesses, however, were connected with ideational constellations.

cent revival which has set the older experience in *Bereitschaft*.<sup>25</sup> The writer has succeeded by persistent effort in tracing all the items of an elaborate dream to the waking consciousness of the preceding day and evening.<sup>26</sup> He inclines to the belief that the primary and chief incentive to reproduction in dreams is furnished by recent cortical functions and that the bizarre and novel character of the dream is owing to the fact that bits are torn from the most diverse interests and topics of the preceding day and composed in a single scene. In spite of this strange conglomeration of items drawn from many sources, however, the dream not infrequently reflects the "personality" of the dreamer; that is to say, his temperament, traits, persistent interests, ethical and social principles, color and temper the dream. Thus A recognizes himself in a tendency to be tardy at appointments, in the wish to work alone, in the dislike of disorder, in sensitiveness to opinion, in contempt for prying persons, in psychological interests, in obstinacy; B finds a fixed habit to economize, a tendency to bashfulness, anxious responsibility for his work, and irritation under criticism. E shrinks from public display, enjoys self-commiseration and is inclined to worry over public engagements. Thus it appears that the dream, although it may be trivial when judged by the day's standards, and although it may be largely occupied with perceptual functions set up by small events of the preceding hours, nevertheless runs in old grooves and is, in part, fashioned by determining tendencies at least as old as the individual dreamer.

Upon the *differences observable between the dream and the waking mind* it is easy to follow the irresponsible judgment of the lay-dreamer and so to generalize. We are told that "people" do not think in dreams, that dreams are wholly irrational, that incongruities are not recognized, that dreams do not pursue a given topic, and what not. In our own intro-

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<sup>25</sup> *E. g.*, the dream-death of an acquaintance of earlier years due to a funeral procession witnessed the day before the dream occurred. Cf. Spiller, G. (*op. cit.*, 449-450) who cites other instances. The veiled connection of the dream with the preceding day is noted also by Hacker (*op. cit.*, p. 113), especially in early dreams. Light morning dreams more frequently forecast the usual day's routine.

<sup>26</sup> It is natural to suppose that the frequency of dreams of nakedness, exposure, shame, and the like, is due to the preceding preparation for bed. Private and domestic scenes woven into public appearances and social encounters do not fail to offend the dreamer's sense of the proprieties. Perhaps many of the sexual motives cherished by the Freudian proceed from this innocent cause.



spectations we found not one of these generalizations to hold.<sup>27</sup> Besides the observations made upon each individual dream-record, the observers attempted, at the end of the week, to draw wider distinctions; but most of these distinctions were either qualified by the individual observer or by a comparison of all the reports when thrown together. Whatever is left over for general formulation may be put in the following words. The waking functions least disturbed are the perceptual functions. The chief disturbance to them is referable to breaks in perceptual trains which are provided against, in waking, by the continuity of environmental changes. The sensory materials of perception are, also, as we have seen, more limited and less various than those provided by the active and alert organism. It is strange that the perceptual functions should be so well sustained under circumstances which make it impossible for the organism,—out of touch with its surroundings during sleep,—to profit by them. The disturbance to the central nervous activities seems, however, less to derange those unknown processes which preserve bits of antecedent function than to destroy the synergetic operations of the cerebrum;—the operations which are necessary to balance and control, and which integrate at once racial and habituated sets, determining and associative tendencies, and the immediate effects of external and organic stimuli. The result for consciousness is that thinking is rare and, as a rule, ineffective; that secondary attention is fitful and incidental; and that those wide integrations which are compassed by the human psychophysical organism in its most productive moments are almost entirely wanting.

Our introspective results are indicative rather than constructive. Even when added to the whole mass of antecedent work upon the subject they do not wholly suffice for the foundation of a doctrine of the dream-consciousness. But the method does, when it is taken seriously in hand by the psychologist, yield results. The writer's experience with it leads him to believe that a person thoroughly trained in introspection can, with practice and with definite instructions, reproduce the dream-consciousness with almost photographic fidelity. At the very least, the method is capable of carrying our knowledge a long way beyond the limits of loose generalization. Moreover, it

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<sup>27</sup> Calkins (1893) came to a like conclusion. The danger of drawing wide inferences from the dreams of a single Obs. appears in S. Meyer (*Zeitschr. f. Psychol.*, liii. 1909, 206), who argues on the basis of his introspections against the hallucinatory character of the dream.

may, by forming an alliance with the external agencies of bodily control, be carried to a much higher state of specialization than we have yet attempted.

As regards the second, the pedagogical aim of the inquiry, the members of the seminary were quite agreed at the conclusion of the task that the method admirably served our purpose. It compelled us to use psychological terms with a common import, it defined and quickened our powers of observation, and it convinced us that the dream-consciousness, cut off as it is from the confused interests and demands of our daylight lives, is adapted in a unique and special way to introspective examination.

## TESTS ON ADAPTIVE INTELLIGENCE IN DOGS AND CATS, AS COMPARED WITH ADAPTIVE INTELLIGENCE IN RHESUS MONKEYS

By W. T. SHEPHERD, Ph.D., Waynesburg College

This paper is a report of experiments which were made with dogs and with cats to ascertain to what degree they compare in adaptive intelligence with Rhesus monkeys. The tests on the latter animals, which have been already reported,<sup>1</sup> were made the basis of the experiments on the dogs and cats and will be restated here. Following that, we shall give an outline of the results of similar tests made on the other animals.

By the term "adaptive intelligence," we may designate a lower variety of reasoning. The term reasoning has been used in a variety of ways, and the following definitions include the most important meanings that have been given the term: *A*, implied reasoning (Harris), e. g., my recognition of yonder horse; *B*, inference from particular to particular (James), e. g., the bird which finds bread upon the window one morning comes back the next morning; *C*, adaptive intelligence, the ability to adapt to our purposes conditions more or less difficult and more or less unfamiliar; *D*, analogical reasoning, which involves construction or creation, e. g., to reach an upper window, I utilize a ladder which I find; *E*, rational thinking (James); *F*, formal or syllogistic reasoning.

With the purpose of investigating the matter of the presence of "adaptive intelligence," as above defined, in Rhesus monkeys, the following experiments were made on those animals:

*Adaptive Intelligence in Monkeys.*—I. A piece of twine was let hang in front of and twelve inches away from the cage, beyond the reach of the longest-armed animal which was tested. At the end of the twine a piece of banana was arranged; a thin piece of wood was pushed through the banana and turned so that one end could be grasped by an

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<sup>1</sup> W. T. Shepherd, Some Mental Processes of the Monkeys, *Psychol. Rev. Mon. Sup.*, XII, 1911.

animal in the cage. By grasping and pulling the stick inwards, the food could be secured. All the eleven animals were tested in this experiment. The results with all but one of the animals were similar, and a description of the actions of the one will suffice to indicate the reactions of all. As soon as the banana and the stick were arranged, monkey 6 put her arm through the wire of the cage, seized the end of the stick, drew it towards her, and secured the banana. This experiment was repeated a number of times and in all there was an immediate characteristic response. There appeared to be a decided adaption of means to end. No efforts were wasted on random movements. It did not appear that any preliminary attempt was made to grasp or even to reach for the food, but there was an immediate movement toward the stick. The results for all animals are given in Table I, in which are shown the approximate time for the performance of the act by each animal in each trial. The absence of hesitation, the direction of the movement away from the food towards the stick, and the promptness with which the food was seized, all speak for the presence of adaptive intelligence in ten of the monkeys. It would seem that it is always found in these animals.

TABLE I

ADAPTIVE INTELLIGENCE. *Suspended food and stick. Three or six trials each animal. Times in seconds; f means failed*

Trial and animals	1	2	3	4	5	6
1	60	10	3			
2	4	3	3			
3	20	6	9			
4	105	40	11			
5	f	f	f			
6	2	1	1			
7	6	4	3	1	1	1
8	6	4	4	3	3	3
9	5	3	3	3	4	2
10	2	1	1	1	1	1
11	5	5	3	3	3	3

In this test the results with monkey 5 were decidedly different from those with the other animals, in that he failed in the trials given him. Monkeys 1 and 4 had considerable difficulty in getting the food in the first and second tests;

but there was no gradual acquisition of the method of securing food in the other eight animals. The times for solving the problem in the second and third tests were approximately the same as those in the first test for monkeys 3, 6, and 10; and there was not much difference in the time between the first and later tests for monkeys 3, 7, 8, 9, and 11. Much if not all the difference in time can be accounted for by the better adjustment to the wire netting of the cage, the pushing of the hand through the proper place, etc.

II. A second test of the presence of adaptive intelligence was made as follows: A light wooden lever 18.5 inches long was attached by leather hinges at one end to a board which rested on the floor. The hinge of the lever was four inches from the end of the horizontal board, well within reach of the animals. The lever was inclined at an angle of approximately 45 degrees from the horizontal, and could be moved forward in a vertical plane. The apparatus was placed outside a cage, the lower end of the lever being within, the upper end being beyond the reach of the animal. A piece of banana was placed at the farther end of the lever, and the problem for the animal to solve was how to secure the food, which was beyond direct reach.

Eleven monkeys were tested with this apparatus, and all with the exception of monkey 4 succeeded in the first trial. In the first tests the animals usually took a longer time to get the food; but as in the previous experiment this delay was largely one of making the adjustment of the hand to the proper opening in the wire netting and not to attacking the apparatus in the wrong manner.

Monkey 4 appeared to be frightened at the time of the first trial and I showed him that food was to be secured by moving the lever against the cage and permitting him to secure the food. In the succeeding trials he immediately attacked the lever and obtained the food in the same manner as the other animals. Monkey 9 was in the cage with Monkey 8 when the latter was being tested and may have taken the opportunity to observe Monkey 8. That she performed the trick may have been due to imitation; but I credit her with having performed it in the same way as the other animals. It is reasonable to suppose that Monkey 4 would have been able to manipulate the apparatus without being shown if sufficient time had been allowed. In many of the experiments the animals did not use both hands for moving the lever, but one hand for moving the lever and the other

for securing the food when the upper part of the lever was within reach. Table 2 gives the time records for four monkeys in this experiment; those for the other animals were approximately the same as those noted in the table, although no accurate measurement by watch was taken.

III. One end of a piece of twine, one yard long, was attached to a bucket and the other end was placed within the cage. The bucket was placed at the distance of the length of the twine from the cage; and a piece of food was fastened to the top of the bucket in sight of the animal. The problem was for the animal to draw in the bucket by pulling on the string and thus secure the food. In all cases, the animals seized the twine immediately, drew the bucket toward the cage and seized the piece of banana.

Table III gives the times for Monkeys 7 and 8. These times are similar to those of the other animals.

TABLE II

TABLE III

Trial and animals	1	2	3	Trial and animals	1	2	3	4	5	6	7	8	9	10
1	9	4	3	7	6	4	3	3	4	3	3	4	3	3
2	6	4	3											
3	2/5	2	2	8	0	3	3	2	2	3	2	2	2	3
4	f	6	3											

The results of the foregoing experiments indicate the possession by monkeys of adaptive intelligence, of a low form of reasoning. The times for the performance of the different acts are reasonably conclusive, but in addition, the appearance of the animals, their actions, etc., especially during the first trial in each experiment, seemed to show an understanding of the problem. There was no fumbling with the apparatus, no appearance of learning by trial and error; but there was instant action following apparently, instantaneous understanding of the situation.

*Adaptive Intelligence in Dogs.*—As a test of adaptive intelligence in dogs, as compared with that shown by the monkeys in the preceding experiments, I repeated some of the tests made with the monkeys. Three individuals were thus tested. Dog 1 was a rather small mongrel male, two and a half years old; Dog 2 was a large male, part hound, four years old;

Dog 3 was a female, medium size, of uncertain stock, three years old. The results with all the animals were negative, and will be reported briefly.

I. The first experiment was similar to the first test made with the monkeys. Food was suspended by a cord in front and just beyond reach of the animal. A stick was passed through the food and one end extended a short distance within the cage, so the dog might seize the end of it with his teeth. By thus seizing the end of the stick within the cage, the animal could draw the food up and within the cage and so secure the piece of meat. It will be seen that the dogs had a slight advantage of the monkeys by this arrangement.<sup>2</sup>

The results, however, were wholly negative. The dogs scrambled about the side of the cage next to the suspended meat, pawed and sometimes bit at the side of the cage, but made no effective attempt to seize the end of the stick and draw in the food. They each appeared to have no understanding whatever of the problem. They all failed entirely in six trials of one minute each.

II. A second experiment similar to the third experiment with the monkeys was made, i. e., a bucket was placed at the distance of a string one yard long, and with a piece of meat fastened to the top, and in sight of the dog, having the loose end of the string within the cage. The dog, if he possessed the adaptive intelligence, could seize the end of the string in his teeth, draw up the bucket to the cage and thereby secure the food.

In this test again, unlike the monkeys, the dogs all failed, apparently showing no understanding of the problem and no ability to cope with it. There was only a general scramble around the cage, and at the side next the bucket, at the sight of the tempting meat. Sometimes they would appear to give it up.

Here again we appear to see the superiority of the monkey intelligence in dealing with difficulties and in adapting conditions to their needs. We must, however, remember the superiority of the monkeys' motor equipment for dealing with this sort of problem. It might not be safe to conclude that a dog could not do equally as well on his own ground, e. g., in a hunting problem. Furthermore, I do not doubt but that, as Thorndike<sup>3</sup> has pointed out, the apparent superior general

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<sup>2</sup> It should be remembered that in a similar experiment with the monkeys the nearest end of the stick was outside the cage.

<sup>3</sup> E. L. Thorndike, *The Mental Life of the Monkeys*, *Psychol. Rev. Mon. Sup.* III, 1901.

intelligence of the primates is largely accounted for by their superior sensori-motor equipment. But I still believe that they possess a modicum of something resembling real mentality, not present in other animals. On an evolutionary hypothesis, we might expect such to be the case.

*Adaptive Intelligence in Cats.*—I. Two cats were used in this experiment. Both were full grown and apparently of fair intelligence, although neither was of very active habits.

Both cats were tested under experimental conditions similar to those in experiments with both monkeys and dogs, i. e., a piece of meat was suspended in front of the cat's cage, with a stick run through it, and as in the case of the dogs, projected through the wire into the cage.

In brief, the results were similar to those with the dogs: There was a total failure to understand the problem, or even to try to seize the stick and pull in the food. The cats scrambled about the cage at the side next the suspended meat, but it never seemed to occur to them to use the stick as a means of drawing in the food. Each animal was given six trials, and two minutes were allowed in each trial.

II. The second test was similar to the second experiment with the monkeys,—that is, the inclined lever with food on the outer end was employed. The cat was to reach its paw through the wire, seize the lever end which was within its reach and thus draw up the food to the cage. The results were similar to the results of the previous experiment. There was an entire absence of any evidence of an adaptive intelligence similar to that shown by the monkeys in a like experiment.

We conclude, therefore, that both dogs and cats are absolutely inferior to Rhesus monkeys in dealing with problems of the character of those herein reported and with experimental conditions of a similar nature. This, however, may be due in part to the superior motor equipment of the primates. It is also possible that under experimental conditions more favorable to them, dogs and cats might show an equal or even a superior ability to that of monkeys. However, the writer ventures to infer that in the matter of adaptive intelligence in general, Rhesus monkeys are much superior to their humbler congeners.



## EXPERIMENTAL STUDIES IN RECALL AND RECOGNITION

By EDITH F. MULHALL, Barnard College, Columbia University

The greater part of experimental work on memory has been on recall. More recently investigators have employed the process of recognition, but few of the differences between the two processes have been reported. The aim of the following experiments was to study the differences between recall and recognition. Tests were made to measure (1) the equivalence of repetitions for recall and recognition, (2) the influence of determination to remember, and (3) the effect of primacy and recency on both recall and recognition.

### *The Equivalence of Repetitions for Recall and Recognition.*<sup>1</sup>

The difference in the number of repetitions necessary for the mastery of fifteen items was determined for four materials, pictures of objects (lamp, fish, clock, etc.), geometrical forms, words (chair, hammer, car, doll, etc.), and nonsense syllables (fik, vod, deb, ruz, biv, etc.).

Each subject was shown fifteen words successively at regular intervals of two seconds each and then required to write those he remembered. A set of thirty (30) containing the original fifteen (15) was given to the subject from which he was to select fifteen which he thought were in the original set. The first set of fifteen was presented again as before and the subject was requested to recall those he could and then to select fifteen from the thirty set. This was repeated until he was able to recall and recognize the fifteen items correctly. The same experiment was performed for all four materials on twenty-five subjects, all undergraduate or graduate students in Columbia University.

Table I shows the average number of repetitions necessary for complete recall and complete recognition, and the ratio of the two for all materials.

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<sup>1</sup> Mulhall. Equivalence of Repetitions for Recall and Recognition, J. of Phil., vol. XI, No. 15, p. 411.

TABLE I

<i>Material</i>	<i>Process</i>	<i>Av. No. Repetitions</i>	<i>M. V.</i>	<i>Ratio Av. for Recall to Av. for Recog.</i>
Pictures.....	{ Recall.....	3.36	.79	3.23
	{ Recog.....	1.04	.08	
Forms.....	{ Recall.....	3.96	.85	2.2
	{ Recog.....	1.80	.81	
Words.....	{ Recall.....	4.76	1.61	1.80
	{ Recog.....	2.64	.96	
Syllables.....	{ Recall.....	7.12	2.23	1.22
	{ Recog.....	5.80	2.11	

The data indicate that the difference between recall and recognition memory is greatest for pictures, somewhat less for forms and words, and least for nonsense syllables. The number of repetitions required for both recall and recognition is least for pictures, greater for forms and words, and greatest for syllables. In examining the materials one finds that the pictures offer the greatest richness of associations. The forms offer somewhat fewer advantages—the shape can be visualized, in a few cases they can be named, and associations, but few in number, can be made. The third material, words, all being nouns, are names and have associations, but lack any particular picture or form element. The syllables, as their name implies, are nonsense; most, if not all, are devoid of any association. Introspections of the subjects indicate that association was employed as an aid in memorizing.

The following secondary experiment shows this more definitely. To each of two subjects a mixed set of sixteen items (four pictures, four forms, four words, four syllables) was presented, one unit every two seconds. Careful introspections were called for as each item was afterwards recalled. The subjects were also required to select sixteen from a set of thirty-two (eight of each material). A few of the records are given as examples of the means the subjects used to aid memory.

<i>Pictures remembered</i>	<i>Why and How</i>
butterfly.....	Benefit Performance of Madam Butterfly to be given for the College.
car.....	Its many uses, with engine, auto, etc.
coat.....	Livery.
bridge.....	A particular bridge in Sullivan County.
lamp.....	Toaster received yesterday which I connected to a lamp.
nuts.....	Walnuts I bought today.

*Words*

tree.....	Curious tree examined on a recent walk.
knife.....	Bread.

*Syllables*

naf.....	Nap.
jod.....	Jodl, psychologist.

*Forms:* The forms reminded the observers of "a diamond," "a windmill," "exclamation point," "color disc," "pie," and "kindergarten shapes."

The introspections of both subjects show clearly the method of association in recalling the material and the difficulty and often inability of remembering material with few or no associations.

Since the greatest difference in recall and recognition memory is for pictures when the material has an abundance of association, less for forms and words, and least for syllables where associations are respectively less, it appears that the difference in recall and recognition memory is in part dependent on the richness of associations present.

*The Influence of Determination to Remember on Recall and Recognition*

The influence of determination to remember on recall and recognition was determined for two materials, one rich with associations, the other devoid of them. Twenty (20) photographs (15 men, 5 women) with names attached, and fifteen (15) nonsense syllables were used. Fifty subjects were tested, twenty-five with no determination to remember and twenty-five with determination to remember the material. The first

group was asked to take part in an experiment in judgment. Each subject read the following instructions:

"Arrange the 15 nonsense syllables in four piles according to ease of articulation. You will be given two minutes to do this."

and was given a set of fifteen (15) cards on each of which was typewritten one syllable, and four cards labeling the four piles, "Very Easy," "Fairly Easy," "Slightly Easy," "Indifferent." If the arrangement was made before the two minutes had expired the subject was urged to make sure the arrangement was satisfactory. At the end of the two minutes the observer was asked to make a record of the syllables in each pile on the blank provided.

Twenty photographs with names attached and four cards labeling the piles "Very Attractive," "Fairly Attractive," "Slightly Attractive," "Indifferent," were given to each observer with the following instructions:

"Arrange the 20 pictures in four piles as indicated. You will be given 2 minutes to do this."

After the two minutes had expired each subject was requested to record the names of the pictures in each pile on the blank provided. This was done to make sure the subject looked at the names under the pictures.

The subject's memory for both materials was tested. Three minutes were given for the recall of the syllables and then a selection of fifteen from a set of thirty, fifteen of which were in the original set, was made. The observer was asked to recall the names of photographs in three minutes. A set of forty photographs, twenty being those of the original set without names, was provided with the following directions:

"Select 20 pictures from this group which you think were previously shown. Name any which you can."

After the subject had selected the photographs, a set of forty cards on each of which was typewritten a name (twenty were the names attached to the photographs in the original set, twenty were other names) was used for the recognition of names. Care was taken not to repeat any name, either first or last, nor to use the last name of any well-known person.

The second group of twenty-five subjects first copied the list of syllables and names on the pictures. This was done in order to control the conditions, keeping them, as far as possible, identical with those of the first group. If such a record

had not been required for the group which was judging, one could not be sure that the subjects read the names. Even with this precaution, the subjects claimed they had never looked at them, until reminded they had written each on the blank.

After making these copies, the subjects of the second group were given the set of fifteen nonsense syllables with these instructions:

"You will be given 2 minutes to look at these 15 syllables. Later you will be called upon to remember them, so look at each with a *determination to remember it*."

After the two minutes had expired, the set of photographs was presented with the directions:

"You will be given 2 minutes to look at these 20 pictures. You will afterwards be called upon to remember the names and photographs, so look at each with a *determination to remember*."

Recall and recognition were tested in the same way as for the first group.

Throughout the experiment an attempt was made to keep the conditions for both groups the same. Any sources of error or awkwardness in conducting the experiment, due to the difficulty of keeping the observers of the first group ignorant of the purpose of the investigation, which may give rise to criticism, will be found to be constant for both groups.

The purpose of the experiment was (1) to compare recall memory, with and without determination to remember, with recognition, with and without determination to remember, respectively; (2) to compare recall and recognition with determination and recall and recognition with no factor of determination present; and (3) (a) to compare recall of syllables, with and without the intention to remember, with recall of names with and without determination, and (b) to compare recognition of syllables with and without the intention to remember with recognition of photographs and names with and without the intention to remember.

In Table II the total number recalled and recognized by both groups is given for both materials. Table III shows the ratio of recall with a determining factor present to recall with no such factor present and the ratio of recognition with a determining factor to recognition without the determining tendency for both materials. Table IV gives the ratio of recog-

dition with determination to recall with determination and the ratio of recognition without determination to recall without determination for both materials.

TABLE II

	SYLLABLES No Determination		Determination	
	<i>Recall</i>	<i>Recog.</i>	<i>Recall</i>	<i>Recog.</i>
Total.....	126	318	155	326
Average.....	5.0	12.7	6.20	13.0
M. V.....	1.65	1.26	2.06	.73

## PHOTOGRAPHS AND NAMES

	No Determination			Determination		
	<i>Recall</i>	<i>Recog.</i> N.	<i>Recog.</i> Ph.	<i>Recall</i>	<i>Recog.</i> N.	<i>Recog.</i> Ph.
Total.....	77	391	461	134.5	435	415
Average.....	3.08	15.6	18.4	5.36	17.4	16.6
M. V.....	.90	1.53	1.48	1.57	1.44	1.90

TABLE III

## SYLLABLES

Ratio of Determined Recall to Undetermined Recall.....	100 : 81.3
Ratio of Determined Recog. to Undetermined Recog.....	100 : 97.5

## PHOTOGRAPHS AND NAMES

Ratio of Determined Recall to Undetermined Recall.....	100 : 57.2
Ratio of Determined Recog. to Undetermined Recog.....	100 : 99.7

TABLE IV

## SYLLABLES

Ratio of Determined Recog. to Determined Recall.....	100 : 47.55
Ratio of Undetermined Recog. to Undetermined Recall.....	100 : 39.62

## PHOTOGRAPHS AND NAMES

Ratio of Determined Recog. to Determined Recall.....	100 : 15.82
Ratio of Undetermined Recog. to Undetermined Recall.....	100 : 9.03

TABLE V

Ratio of Det. Rc. of SYLLABLES to Det. Rc. of NAMES.....	100 : 86.8
Ratio of Undet. Rc. of SYLLABLES to Undet. Rc. of NAMES..	100 : 61.1
Ratio of Det. Rg. of SYLLABLES to Det. Rg. of NAMES and PHOTOGRAPHS.....	100 : 260.6
Ratio of Undet. Rg. of SYLLABLES to Undet. Rg. of NAMES and PHOTOGRAPHS.....	100 : 267.9

In Table V the materials are compared. The ratios of recall of syllables to recall of names are presented with no determination and with determination to remember; the ratios of recognition of syllables to recognition of photographs and names with and without determination to remember are also stated.

It will be noticed that fewer photographs were recognized when there was a determination to remember present, but many more names. This does not mean that determination to remember decreases one's ability to recognize faces. The larger number of names recognized with determination to remember seems to indicate that the observers considered names more difficult to remember and spent most of the time allotted to them to learn names. The total number of photographs and names recognized with determination to remember was 850 and without 852, or an average in each case of 34 items. Throughout, the calculations involving the recognition of the material with greater meaning the totals 850 and 852 have been used. Frequently a subject could recall the first or last name but not both; in each case the score of one-half was given (Table II).

TABLE VI  
RECALL OF NAMES

	No Determination			Determination		
	1st only	2nd only	Both	1st only	2nd only	Both
Total.....	40	50	32	49	62	79
Total First Names.....		72			128	
Total Last Names.....		82			141	

The data indicate the following: (1) the factor of determination to remember ~~influences recall memory~~, but its effect on recognition is little, if any; (2) the difference between recall and recognition is less when there is a determination to remember the material than when there is no intention to remember; (3) the influence of determination for the recall of names is greater than for the recall of nonsense syllables; there is little, if any effect of determination to remember on the total number of items recognized of either material.

A further study of the data for the recall of names shows interesting results. When no determining factor was present 72 first names were recalled and with determination 128, or an increase of 77 per cent; with no determination to remember 82 last names were recalled, with intention present 141, or an

increase of 72 per cent; with no determination total number recalled 77 and with determination 134.5, or an increase of 75 per cent. Determination to remember seems to influence the recall of first and last names to the same degree. Determination to remember influences greatly the number of first and last names correctly connected, as there were only 32 with no determining factor and 79 with one, or an increase of 147 per cent. Moreover, the determining tendency influences the number of photographs which may be correctly named. Table

TABLE VII  
NAMES CORRECTLY ASSOCIATED WITH PHOTOGRAPHS

	No Determination				Determination			
	1st	2nd	Both	Total	1st	2nd	Both	Total
Total.....	2	9	13	18.5	10	24	48	65.0
Highest Score...	1	3	4	4	3	4	7	8
Lowest Score....	0	0	0	0	0	0	0	0

VII shows the number of first, last, and whole names which were correctly assigned to the photographs for both groups of subjects. According to the method of scoring adopted, 18.5 names were correctly given to the photographs by the first group and 65 by the second.

### *The Effect of Primacy and Recency*

The third part of the investigation was concerned with determining the influence of primacy and recency on recall and recognition. Thirty-eight subjects were tested for two materials, nonsense syllables and photographs. To nineteen, a list of twenty-four nonsense syllables was presented at the rate of one item every two seconds in a given order 1 through 24. Immediately after the presentation they were asked to recall all they could in three minutes and then were given a list of forty-eight from which to select 24 which they thought had been previously presented. The subjects were then shown twenty-four pictures at the rate of one every two seconds. As each was presented a name was pronounced by the experimenter. Immediately after the presentation, they were given three minutes in which to recall the names. A set of forty-eight photographs, containing the original twenty-four, was used for the recognition test. Each subject was then provided with a list of forty-eight names from which to select twenty-four which he thought had been pronounced as the pictures were presented. The experiment was repeated on



the second group of nineteen, but the photographs were shown first and the syllables second, and the order of each material was 13 through 24, 1 through 12.

The results were calculated according to the position of the item, *i. e.*, for position 1 the record is given for the number of times item 1 was remembered by members of the first group, and item 13 by members of the second group; position 2, the number of times item 2 was remembered by the first group, and item 14 by the second group; and so on until position 24 shows the number of times item 24 and item 12 were remembered by the first and second groups respectively. (Table VIII.)

TABLE VIII (see Charts)

## PRIMACY AND RECENCY

Position	Syllables		Photographs and Names		
	<i>Recall</i>	<i>Recog.</i>	<i>Recall N.</i>	<i>Recog. N.</i>	<i>Recog. Ph.</i>
1	23	36	3.5	31	27
2	16	34	0	14	19
3	19	34	5	34	28
4	14	27	3	29	17
5	9	32	3.5	29	20
6	10	19	4	29	22
7	6	30	2.5	30	20
8	2	28	4.5	24	32
9	10	29	2.5	24	20
10	8	26	3.5	21	16
11	8	28	4.5	24	27
12	6	27	2.0	24	17
13	11	30	7.5	26	28
14	11	26	.5	19	15
15	9	19	9.0	31	26
16	10	23	4	23	12
17	6	28	4	22	25
18	5	20	3.5	22	25
19	10	27	2	21	24
20	10	20	6.5	29	29
21	6	22	5	20	15
22	19	32	11	24	18
23	11	22	20	35	32
24	21	31	30.5	33	13
Average	10	27	5.9	25.7	21.9

Since one is able to recognize much more than one can recall, any factor which increases recall memory by one unit may not have the same influence on recognition memory when it, too, is increased by one item. The relation of the total number recalled for each position to the average number recalled of all the positions is used as a measure of the effect of position for recall and the relation of the total number

recognized for each position to the average number recognized is used as the measure of the effect of position for recognition. The accompanying charts show the influence of primacy and recency for both processes and for both materials.

Primacy and recency influence both recall and recognition of nonsense syllables, both are more effective in the former than in the latter case. As far as the data of this experiment indicate, the influence of primacy and recency is about the same. Turning to the chart for names and photographs, one notices no effect of either primacy or recency for recognition. Primacy has only a slight effect, if any, for recall, but recency shows a marked influence on recall.

The results will allow only suggestive conclusions, but it

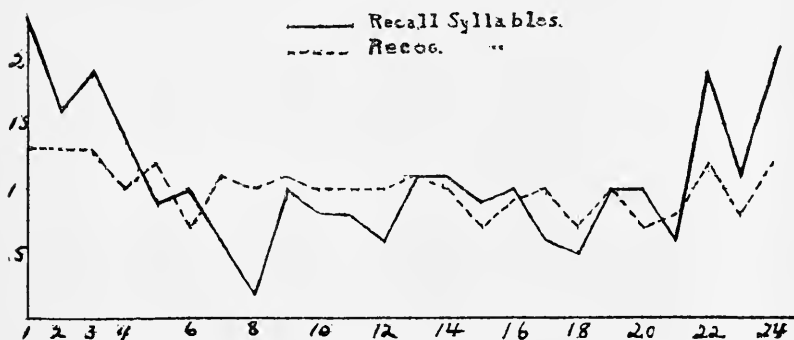


CHART I

Horizontal Scale: Position of the item. Vertical Scale: Ratio of the number remembered for each position to the average.

appears that primacy and recency influence recall memory more than recognition. The influence of both on recognition is greater the less meaning the material has.

The results of experimental studies in memory may lead to practical applications. We have long known that the value of a single presentation is greater in recognition than in recall, and we may add that the difference in the value of repetitions is greater for material with meaning and less for material without meaning. Advertisers will find it takes fewer presentations for prospective customers to recall and recognize a trade-mark which has a wealth of associations.

Banks are eager for competent detectives who will recognize and often associate the correct name to each man and woman who is a depositor. The individual differences shown in

Table VII indicate that people vary in their ability to identify and name faces. A test similar to this might serve as a means of selecting competent bank detectives.

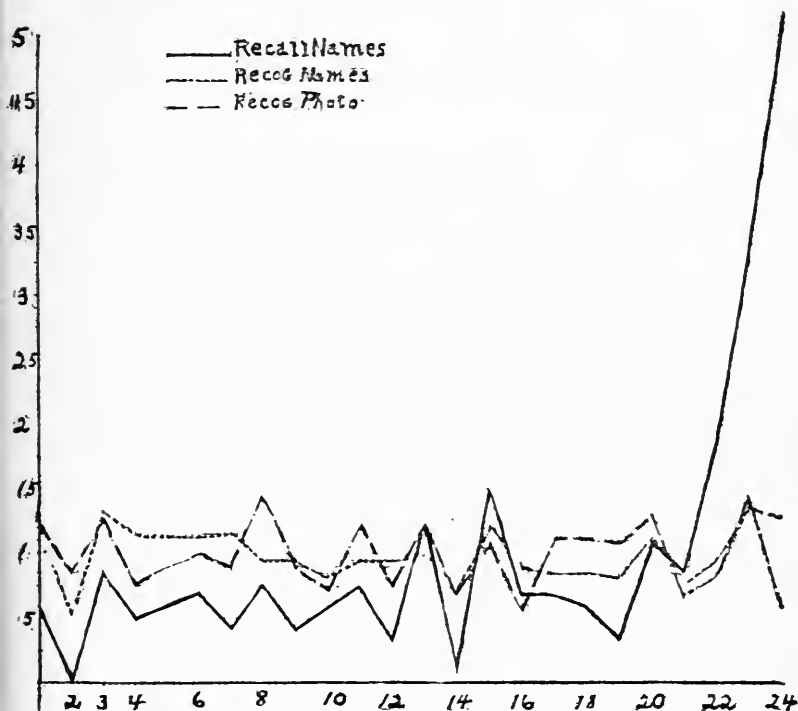


CHART II

### Summary

In general, the difference between recall and recognition varies with the materials remembered and with the same material when different influences are present.

1. The difference between recall and recognition is greatest when the material is rich with associations and becomes less the fewer the associations in the material.

2. Determined recall differs from undetermined recall more than determined recognition differs from undetermined recognition.

3. The difference between determined recall and determined recognition is less than that between undetermined recall and undetermined recognition.

4. The influence of a determining factor is greater for recall of material rich with associations than for material devoid of them.

5. The determining factor influences the amount of material remembered which can be correctly associated with other material remembered.

6. Primacy and recency both influence recall memory. The influence of each on recognition is less than on recall, but is greater for material devoid of associations and less for material rich with associations.

## NOTE ON THE PSYCHOLOGY OF SHAME<sup>1</sup>

By THEODATE L. SMITH

In the field of emotion experimental psychology has as yet offered us but little, for only the simplest emotions have proved available for laboratory investigation, and the results of plethysmographic experiments have thus far not been commensurate with the amount of labor bestowed upon them and are lacking in definiteness. In the present study, while introspection has, of course, been the final test, the material has been drawn from anthropology, animal psychology, and the study of children and defectives. Literature and biography have also furnished illustrative material. I have found but one monograph on the subject, that of Hohenemser, which is written entirely from the introspective point of view, and his four possible types of shame are more strongly suggestive of logical than psychological possibilities and apply only to a highly developed self-consciousness.

Most psychologists seems to have overlooked the existence of shame as a separate emotion, or, at most, devoted a line or two to it in connection with allied mixed emotions. Ribot places modesty, shame, and shyness in a group of emotions which he characterizes as "based on an association of intellectual states, which is, in most cases, an association by contrast," and "presupposes a fusion, in varying proportions, of agreeable and disagreeable states." He further adds that the emotion, as a whole, differs from the sum of its constituent elements, which analysis can describe and isolate. Bain resolves shame into a dread of being condemned or ill-thought of by others. ("Emotions and Will," p. 211.) James finds the origin of the emotional states of modesty, shame, and shyness in the application to ourselves of a judgment previously formed upon others. Baldwin defines shame as a "lowered self esteem, felt with reference to something positive pertaining to self and open to the knowledge or opinion of others."

The motives most closely allied to shame and frequently confused with it are shyness, bashfulness, embarrassment, mortification, humiliation, coyness, and modesty; indeed the

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<sup>1</sup> An unfinished paper found among the author's manuscripts after her death on February 16, 1914.

German has but one term for shame and modesty, yet modesty, whether we take it in its broader definition of "the form of timidity or shyness due to reflective self-consciousness," or the narrow anthropological definition of "physical self-respect," is not in itself a distinctively painful emotion, since unpleasantness arises only in connection with any violation of it. The confusion is indeed due to anthropology rather than to psychology, and is connected with the theory advanced by some anthropologists and by Havelock Ellis that modesty and shame have their origin in the sex instinct.

To the writer, shame introspectively considered seems to consist of a special case of what Lipps has described as *psychische Stauung*, a sort of paralysis which occurs whenever two or more tendencies, each of which tends to appropriate the psychic powers, are active, thus producing, at the point of inhibition, an accumulation of psychic force. There is always present in shame a disharmony in the content of consciousness, a feeling that the social self has lost value. It is not always fear of the opinion of others, for it is possible to be ashamed before one's self, when there is no question of the knowledge of others. This psychic paralysis is paralleled in the physical manifestations of shame, which are muscular weakness, sinking of the head, drooping of the eyes and whole body, accompanied by vaso-motor reactions, which oftenest take the form of flushing, but are sometimes manifested by pallor. In little children the tendency to hide, also manifested in fear, is apparent.

If this analysis is correct, then the capability of experiencing shame must be directly dependent upon the development of the social self-consciousness. In animals, we should expect to find it manifested only among gregarious types or those more or less domesticated and dependent upon human care and companionship; in children, as a somewhat later development than the more primitive emotions of fear and anger—and in defectives in proportion to the degree to which they are capable of becoming socialized. Is this in accordance with the facts? Observations on 814 cases of shame in children from 2 to 15 years and 402 reminiscent cases in adolescents were collected and analyzed. No cases of shame were reported in children under two years, though Preyer mentions one very doubtful example in his son at 10 months. His first positive record, however, is at 26 months. The occasions of shame in young children are very clearly reflections of their environment, typical examples being muddy clothes, dirty face, being seen with a nursing bottle after

having learned to eat, a hole in the stocking, sores on the face, being scolded in the presence of others, etc. In most of the cases it was stated that the child had either been laughed at, shamed or compared unfavorably with some other child. In children a little older, wounded pride in many forms appears. The occasion may be bodily defects, shabby clothing, awkwardness, any reproach of family or station, ridicule or expressions of disgust, social blunders or violations of etiquette. In adolescence, these same causes are frequently greatly intensified and questions of dress, manners, bearing, etc., become matters of extreme importance as sex attraction develops. The moral aspect of shame, which appears to be wholly lacking in the earlier manifestations and more or less confused during childhood, may now become very acute. Yet the fact that the moral quality in shame is not an essential characteristic is evidenced plainly by the fact that our social blunders often cause us keener suffering than do infractions of the moral law. How completely shame is conditioned by the social self is well illustrated in the following case: A boy of 14, whose mother was ill, stayed away from school to help her with the housework. He did it willingly and was rather proud of being able to help so much. But some of his schoolmates stopped to inquire the cause of his absence and found him washing dishes. The situation was immediately changed, and according to his own description, he could not have felt more ashamed if he had been caught "breaking the whole ten commandments" than he did at being caught in the, under the circumstances, morally meritorious act of washing dishes.

In considering animal shames, only those cases were retained as legitimate examples, in which fear of punishment as a possible explanation of the behavior was excluded. One or two examples will suffice to show the character of the material. A pet cat of rather a jealous disposition was shown a life-sized colored paste-board cat. It was rather a realistic representation and being carefully held so that the cat could not see the back of it, was slowly moved towards him. He growled, lashed his tail, and finally sprang at the paste-board imitation, knocking it over and revealing its falsity. Tail and ears drooped and he slunk away under a sofa. He could never again be induced to take the slightest notice of the counterfeit, turning away his head if it were thrust upon him.

That dogs are sensitive to reproach and to ridicule is attested by every careful observer of animals. Shaler gives the following account of a shepherd dog belonging to him

but from which he was often separated by several months' absence. "When after one of these absences I appear to him in the distance he comes furiously barking toward me, quite possessed by his enmity. At a certain point in his charge, a doubt appears to beset him: he moderates his pace; his roaring bark passes into a whine; and as the full measure of his blunder is borne in upon him by my voice, he becomes the picture of shame. In his perplexity, he always finds relief by endeavoring with his paw to scrape a supposititious fly from his nose. He then deals with what I suppose to be an equally imaginary flea. After he has thus gained a few seconds for readjustment, he welcomes me joyously." That reactions of a similar nature occur in a social environment of the animal's own kind is shown by the careful observations of Mr. Ordahl, who thus describes a common occurrence among his cattle when first brought in the fields. "Two young calves are testing their strength, one is pushed out of the ring; the victor raises his head stiffly and jerks his ears alternately; his eyes roll with a gleam of satisfied superiority, while the loser lowers his head, eyes are cast down and the ears hang down relaxed. This lasts for a few moments, when the matter is settled either by the loser cautiously approaching the winner in an attitude of recognized subjection or the winner administering an extra bunt as if to assure himself that the victim is a non-resisting medium."

It is in the social situations arising among gregarious animals that Lloyd Morgan finds the germs of the development of the sense of personality—and Dr. C. W. Hodge, in a paper on "The Intimations of Self-Consciousness in Animals," concludes that we must allow to dogs at least an obscure form of self-feeling. But if we admit even a rudimentary self-consciousness in animals, we have the possibility of an abasement of self which, however crude, may be considered a germinal form of shame and possibly not very different from the earlier manifestations of shame in children. Some investigations of shame in mentally defective children, not yet completed, tend also to show that their capability of feeling shame is in close correspondence with their capability of entering in social relations.

Turning to anthropology for its evidence in regard to the development of shame, we find it, for the most part, confused with modesty. Many of the studies of modesty are bound up with those on the origin of clothing and the chief point at issue is whether modesty was a factor in developing clothing or whether clothing was a chief agent in the development



of modesty. The weight of evidence seems to favor the assumption that psychological modesty preceded physical, and that clothing has been instrumental simply in the development of conventional modesty. This opinion is supported by Ploss, Stevins, Westermarck, Wallace, Foster, Rowley, and Zimmermann, who think that modesty exists quite as truly among unclothed as among civilized peoples. But even in tribes where so far as external evidence goes, modesty is apparently unknown, we find abundant evidence of shame, arising from precisely the same classes of causes or occasions as among civilized peoples. Westermarck, in his "History of Human Marriage," says: "Ideas of modesty are altogether relative and conventional. Peoples who are accustomed to tattoo themselves are ashamed to appear untattooed, peoples whose women are in the habit of covering their faces consider such a covering indispensable for every respectable woman; peoples who for one reason or another have come to conceal the navel, the knee, the bosom, blush to reveal what is hidden. It is not the feeling of shame which has provoked the covering but the covering which has provoked the feeling of shame." Ploss states that it is extremely difficult to obtain exact information in regard to the crippled feet of Chinese women as they are unwilling to allow even a physician to see them. A Chinese woman considers it improper to allow her husband to see her uncovered feet. It is only the tip of the foot encased in its miniature shoe that constitutes the admired "golden lilies" of the Chinese, and the ugly mass of deformity above, which is the penalty of these admired ornaments, is something not willingly exposed. It is possible that we have here a connection between modesty and the æsthetic feeling, since we frequently find the tendency to conceal associated with whatever may be regarded as ugly, repulsive or disgusting.

Darwin in his chapter on blushing ("Expression of Emotion in Man and Animals") records a number of examples of shame among primitive races and notes the sensitiveness of the Maoris to ridicule and cites the case of an old man who, when laughed at, blushed up to the roots of his hair. A Fuegian blushed when quizzed about polishing his shoes and otherwise adorning himself. An Arab on the Nile blushed quite up to the back of his neck when laughed at by his companions for managing his boat clumsily. Rusden in his history of Australia, in describing the corroborrees of the Australians says that "the perfection of acting was aimed at by each man in the tribe," and that the unlucky wight who

committed a mistake in the public performance or missed a turn in the air was followed by the taunts of the tribe for weeks afterward. From this it would appear that rules of etiquette are quite as binding and violation of them quite as humiliating among Australian aborigines, after their own fashion, as among civilized peoples. Many other savage customs point toward the origin of shame in the social consciousness rather than in any biological facts.

The development of self-consciousness or the conscious self is for the individual a very gradual process. It begins with the bodily self. For the new-born child the universe is a series of disconnected sensations to which neither the idea of subject or object is attached. There is a pleasure-pain quality in these sensations, so far as may be judged by muscular expression, but the child does not yet know that the body which experiences these feelings of comfort and discomfort is his own. A large part of the first year of life is spent in making this discovery and even then it is by no means complete, as shown by the studies of Preyer, Hall, Shinn and others.

But before the bodily self and not self have been completely differentiated a dawning self-consciousness has already expressed itself in emotion and desire. The more primitive emotions—fear, anger, jealousy, and animosity, have appeared, and the beginnings of a social consciousness are shown in the child's responsiveness to the influences of environment, to approval and disapproval. According to Dudley Kidd this is earlier among the children of civilized than those of primitive peoples. A Kafir child will beat or bite the blanket of a person with whom he is angry and a big boy will cry if his clothing or possessions are beaten. He also confuses himself with his shadow, his name or his picture. His personality too is mixed with that of his clan. This would account for the shame and anger exhibited when the honor of the clan is violated.

With all these stages of the development of self-consciousness we find the various types of shame in man and animals closely corresponding. The immediate causes or occasions of shame vary with environment, and the intensity of the emotion varies as the development of sense of self in the individual. A moral quality appears to be attached only to types sufficiently developed to admit of a conflict between an ideal self and some content of consciousness which is in disharmony with it. But that the feeling of shame is not confined exclusively to the violation of convention among primi-

tive peoples but has an ethical value in some cases highly developed, is shown in the following case of a Navahoe Indian who, wishing to convince his interlocutor of his truth, uttered the following words: "Why should I lie to you? I am ashamed before the earth: I am ashamed before the heavens: I am ashamed before the dawn: I am ashamed before the evening twilight: I am ashamed before the blue sky: I am ashamed before the darkness: I am ashamed before the sun: I am ashamed before that standing within me which speaks with me. Some of these things are always looking at me. I am never out of sight; therefore I must tell the truth. That is why I always tell the truth. I hold my word tight to my breast."<sup>1</sup>

As to the components which enter into the emotion, its physical manifestations and many anthropological facts point to fear, disgust, and perhaps violations of the æsthetic feeling as well as modesty as the roots from which it has developed. Genetically, then, both the physical manifestations of shame and many anthropological observations tend to show that it is a development from the more primitive emotions of fear and disgust, and violations of æsthetic feelings as well as modesty developed as to its special form through the reciprocal action of environment upon the consciousness of self.

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<sup>1</sup> Washington Matthews: *The Study of Ethics Among the Lower Races*. J. of Am. Folk-Lore, Vol. 12, pp. 1-9.

## THE ORIGIN OF LAUGHTER<sup>1</sup>

By SYLVIA H. BLISS

"The greatest of thinkers, from Aristotle downwards, have tackled this little problem, which has a knack of baffling every effort, of slipping away and escaping only to bob up again, a pert challenge flung at philosophic speculation." *Laughter, An Essay on the Meaning of the Comic.* Henri Bergson.

There is noticeable in various quarters a revival of interest in the subject of laughter. Thinkers are resolutely attacking this problem—which Bergson may term little, but which in reality is very large. Our knowledge of the exciting causes of this most human act has been greatly extended, yet the subject is by no means exhausted. Despite the clever and penetrating theories advanced the essential secret of laughter remains shrouded in mystery.

Is it not possible that an attack from a different quarter may prove less barren of results? The question may not be for the philosopher after all but instead demand evidence from the biologist and the psychologist. If, through all the centuries, the challenge has been flung at the feet of the wrong party, Bergson may well characterize it as "pert." The problem is not primarily in regard to the nature of the thing laughed at; neither has it to do in the first instance with the reason for laughter at that particular thing. In the act itself is the mystery. Why should we laugh at all?

Those who have investigated the matter have too often paused in their search when a perception of one aspect of the comic has been attained. Thenceforth their endeavors are directed to the task of elucidating and illustrating this one aspect with the result that the original, underlying cause of laughter remains almost untouched. The earnest student turns from current theories in disappointment. They do not go deep enough. Dr. Sidis' recent book, with its wealth of illustration—which his theory, "Allusion to human stupidity is the root of all comic," is strained to explain, leaves the

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<sup>1</sup> The writer has learned that the argument in this article is similar in some respects to that used by Freud in his treatise on wit. Her investigation was independent of that of the latter.

reader far from satisfied, while Bergson's clever exposition of the comic is based upon the laughter of highly developed, modern man. From the essay of the latter floats the laughter of the cultured Frenchman—late product of the world's most highly civilized race. For an adequate investigation the consideration of quite other and simpler phenomena is necessary.

The apparent causes of laughter are so various that the generalization which embraces all its manifestations must be wide indeed and likewise very simple. We are forced to reckon not only with the broad and generally recognized field of the humorous and with the stock instances of the comic; with deformity, eccentricity, surprise, queerness, "the mechanical in the living," and the inferior, but also with numerous occasions not readily classifiable which yet can by no means be ignored. The laughter of childhood; the laughter with which, according to a press dispatch, two women in the Italian quarter in New York watched the death struggles of a man whom their companion had murdered; the laughter, as noted by the dramatic critic of *The Westminster Gazette*, invariably won from an audience by the utterance on the stage of the word "damn"—these are a few of the instances awaiting accurate classification.

The evidence afforded by the smile must also be admitted. This fleeting "pale vestige of a laugh," arising at a multitude of provocations, supersedes to a great extent in the highly cultivated person, the more vigorous physical reaction.

Science has demolished the idea of a "funny bone," finds in the brain no special center for appreciation of the humorous, and in the world of thought and action nothing inherently, intrinsically comic. A social corrective, an outlet for surplus energy, relief from strain—laughter is all these assuredly, yet to perceive certain ends which a function serves is by no means to account for its existence. It is evident that the laughter of the modern human being is a highly complex function far removed from the bald simplicity of that first laugh whose strange sound broke the long, silent gravity of the pre-human ages. What emotion gave rise to the first laugh? Was that primeval man in any modern sense amused? He laughed at no sally of wit; his brain was tickled by no subtle and devious perception of the comic. Of this we may be certain. Summoning to our aid the evidence which biology and psychology afford and guided by the imagination exalted by scientific training to a species of insight, we will penetrate to that far away epoch, when mirth was born to earth.

What was the first emotion, and what its cause? Without

doubt the organisms lowest in the scale of life were and are emotionless. Fulfilling without hindrance or delay their functions of nutrition and reproduction, with need and opportunity for satisfaction, income and expenditure, nicely balanced, what room for pleasurable or painful feeling? Scarcely with accuracy may we use the term satisfaction in connection with forms so simple, for satisfaction implies a period of want, and here such hiatus between desire and fulfillment, if existent, is infinitesimal. With advance in the scale of life this simple equilibrium is disturbed. Instinct is delayed in its functioning, hunger and reproduction must wait on occasion. Thus began the swing of the pendulum between need and satisfaction, and without doubt all the marvelous complexity of human emotion is due in the last analysis to the lengthened repetition, the slower and more devious recurrence, of that primal oscillation. With further advance need grew more poignant, satisfaction more intense, until in the higher animals we meet states of feeling fitly termed sad and joyful.

Why not laughter? Why is man alone, of all creation, the animal that laughs? Readily enough comes the answer: Because man alone has intelligence to perceive the humorous. But we find that intelligence is not coextensive with laughter nor is appreciation of the comic a necessary condition of its arousal. Yet it is possible that a consideration of certain other differences between men and animals—differences usually overlooked—will yield matter of greater significance to our problem.

The animal is perfectly natural. It follows instinct, hiding and repressing nothing. It may growl, roar, fight, give chase, plunder, excrete, and, subject to certain limitations, feed and reproduce, when it wills. Indecency and shame are words without meaning. Rightness exhorts no homage. If he loves, well and good, but if he hates no social or ethical code torments him with its "ought." In scientific phraseology there is for the animal no inhibition of instinct. With this free and natural state contrast man's condition. In the evolution of humanity those instincts which are inimical to the progress of civilization are theoretically transformed into qualities and acts less at variance with social and ethical laws, but in reality the substitution is far from perfect and entire. Man is not yet completely evolved; he is but partly adjusted to a civilized environment and a portion of his nature lags far behind at a primitive, savage level.

The human being, from childhood up, must curb, repress, skulk, hide, control. From the mother's "no, no," to the

thundering "Thou shalt not" from Mount Sinai there is a constant denial of instinct. So accustomed are we to regard this as pure benefit that we are blind to the accompanying disservice.

Consider man's inner life—a nest of hopes, impulses, and desires, in themselves perfectly natural and to be expected at this stage of evolution, yet directly in opposition to the prevailing social, ethical and religious codes. Nature confined is not entirely quiescent. With all the outlets, transformations and substitutions which physical and mental activities afford there remains still a large residue of repressed primal instinct which results in discordant and tense conditions in the subconscious life. The repressions of the primitive man of our study were obviously of the most elemental impulses and took place in obedience to nascent social demands of the tribe and through his own growing sense of shame.

Regarding emotion in its essence as the state of tension occasioned by delay between impulse and act, desire and fulfillment, hunger and satisfaction, and perceiving that the primary emotional difference between animals and men consists in the fact that the latter, of their own volition, to serve certain ends, carry on the work of restraint and delay initiated by nature, eventually pushing into the subconscious region tendencies at variance with the slowly forming social code, we discern the conditions ripe for the origin of laughter. For laughter is the result of suddenly released repression, the physical sign of subconscious satisfaction. Our primitive man saw, it may be, another do the thing reprobated by the tribe and his own nascent conscience. The sight relieved the tension occasioned by his own repressed wish to do the selfsame thing—and he laughed. That paroxysm of nerve and muscle was not caused by sense of superiority, as Dr. Sidis would no doubt claim; rather it sprang from unconscious sympathy with the reactionary act.

It is probable that the track for that particular discharge of nervous energy was laid long before actual laughter took place. Darwin noted that the lower jaw of some species of baboons quivered up and down when they were much pleased. The writer has seen the jaws of cats quiver when the animals were excited by the near approach of prey. Expectancy, pleasurable anticipation, satisfaction, with their accompanying bodily reactions, paved the way for laughter.

Today in the highly civilized human being what part of the personality laughs? The highest and latest evolved? The keen trained intellect perceiving something which delights it?

Do wisdom, learning, dignity, purity, laugh? Recall Meredith's opinion as voiced by Adrian Hartley: "Mournful you call it? Well, all wisdom is mournful. 'Tis therefore that the wise do love the Comic Muse. Their own high food would kill them. You should find great poets, rare philosophers, night after night on the broad grin before a row of yellow lights and mouthing masks. Why? Because all's dark at home." From what region does that broad grin come? From idealism, philosophy and wisdom satisfied by nonsense, pleased at folly? Or does the laugh spring from that in the man which is akin to what is portrayed?

We are but half in sympathy with that which we profess. We are not in all respects what we think we are. Under certain conditions there is release of our unrecognized tendencies. De Quincey said that a man is disguised by sobriety rather than by intoxication; and as intoxication uncovers the under-self in man, so laughter, in the temperate, discloses, more delicately and deviously, the mind's unconscious tendencies. Grave, dignified, humane, respectable, religious—why do we laugh at the clever portrayal of characters quite the opposite? Is it not because a part of our personality is in sympathy with the indecorum, triviality, vulgarity of what we see? Are we not as honest at the vaudeville as at church—if we attend both? Even more honest? There may be hypocrites at church; never at the theater. We may bow the head during public prayer because it is the custom; we laugh at a joke because we must.

This view finds confirmation when we consider what occurs during a struggle to suppress laughter—at the indecorum of a child or at a joke "delicately not decent." Our surface sense of propriety strives to suppress the evidence of subconscious approval. Without doubt the blush is the result of similar though still deeper, conflicts.

A clever comedienne responding in a recent interview to the query, How is one funny? said, "I believe that the thing that really takes is the thing with a touch of nature in it." Her remark was more profound than she knew. The secret of laughter is in a return to nature. Civilization and culture are late additions and we are living to a great extent in artificial conditions. Even common sense, according to Bergson, is an effort. Psychology makes plain the fact that our present mental equipment has been slowly and painfully acquired and a certain strain in maintaining that high altitude is inevitable. This tension is relieved by nonsense and by the portrayal in humorous anecdotes and on the stage of evasions of con-



vention and infractions of the prevailing code of manners and of morals. Carlyle once declared that Shakespeare was greater than Jesus and pointed to the creation of Falstaff as proof of his assertion. Of Christ he said: "There is no Falstaff in Him." For the individual who is not shocked by this remark it is a perennial source of humorous feeling. I defy a broad and healthy-minded man, even one of fine religious temper, to read it without smiling. Casting about for a reason what do we find? Contrast, the inferiority of Falstaff, the fact that he is a stock character, typical of certain vices? Various formulas of the comic fit the case, but do not, I venture to say, explain the peculiarly humorous character of Carlyle's statement. Falstaff stands as a representative of the primitive, natural man, and brought into juxtaposition with his most startling and exalted antithesis in character, elevated for once in human judgment above that which has stifled and repressed the traits he represents, he triumphs for the moment, and through him nature in ourselves justifies its existence—and laughs.

I am not aware that any explanation has been given of the smile of the subject entering the hypnotic state. As this is a usual accompaniment of the condition it presents an interesting problem. If the theory advanced in this essay approaches the truth the solution of the problem is made plain by Professor Jastrow's description of the hypnotic consciousness: "— a release from the restraining influences of fear, hesitation, and the ideals of reason and propriety." Here again a simpler, more primitive self has its opportunity—and smiles.

Having stated, somewhat clearly and convincingly, it is hoped, the theory of laughter as the expression of subconscious satisfaction, it remains to test the theory by application to various occasions of laughter. Before proceeding, however, a glance at Bergson's essay on the subject will be instructive. As I advanced in my study from point to point, the main thesis becoming at each step ever clearer and more reasonable, I was struck by the confirmation afforded it by certain isolated passages in the work of the French philosopher—passages which he interprets in accordance with his definition of the comic as the "mechanical in the living." "What is essentially laughable is what is done automatically. In a vice, even in a virtue, the comic is that element by which the person unwittingly betrays himself—the involuntary gesture or the unconscious remark." "Profoundly comic sayings are those artless ones in which some vice reveals itself

in all its nakedness." "Any incident is comic that calls our attention to the physical in a person when it is the moral side that is concerned." "The comic character slackens in the attention that is due to life." "The comic is that side of a person which reveals his likeness to a thing."

Why should we laugh at automatism, the mechanical, absent-mindedness? Bergson's explanation is that these things stand for unsociability, for imperfection, which call for the corrective of laughter. But we do not laugh in order to correct the comic. Laughter is spontaneous and involuntary. It bursts forth when a person unwittingly betrays himself because that betrayal is of natural, elemental tendencies. Laughter is honest and responds to the primitive honesty of others. We laugh at a sneeze which hinders a speech at its most pathetic passage because pathos in oratory is always somewhat strained—even artificial, while the sneeze is absolutely natural and honest. We laugh at the sudden intrusion of the physical when the moral, spiritual, or intellectual is predominant because the latter are of late date in man's evolution—from the body's standpoint interlopers in the household of personality, and the abrupt resumption of rule by the physical delights because there is relief, triumph, even, fancifully speaking, a species of revenge. What has not the body suffered through dominance of the soul!

Passing to the consideration of some of the conditions which give rise to laughter it must be admitted at the outset that not all upon analysis yield to the interpretation advanced in this essay. But let it be remembered that our theory endeavors to account for pure, elemental laughter. Once organized, once this particular emotional reaction—possibly accidental in the first instance—became a habit of the nervous system, occasions for its use multiplied. The act was of value to developing humanity. It has eased—it is easing—the transition from the freedom and naturalness of animal and savage life to freedom *from* the animal and savage, our long and difficult task.

All theories of laughter and the comic are more or less applicable for the laughter of the world today flashes and gleams like the rays of light from a many-faceted jewel. Still do we find it in large measure the symbol of subconscious satisfaction but it has become expressive also of various other satisfactions, complex, subtle and devious. Certain of these are direct outgrowths of the more primitive forms and traceable to the same roots, while others are apparently purely mental in character. It is notable that the higher we rise in

the scale of wit and humor the less hearty and boisterous is the accompanying laughter. It has been said that it is with their minds that Americans laugh most, nowadays. Interpreted from the standpoint of our study this indicates an advance in evolution—the substitution of mental for more primitive needs and tendencies.

Obviously our formula covers all instances of the humorous which relate to socially and morally reprobated acts and conditions. It is in this region that the greatest repressions occur and these in turn give rise to our broadest and coarsest humor. The theory likewise accounts for laughter at remarks having double meaning and at jokes whose point depends upon their vague hint or suggestion of evil. These may win a smile from persons who would turn in disgust from obvious vulgarity and obscenity. Only a keen and delicate shaft can penetrate their surface conventionality and refinement. But a step removed is the laughter which greets profanity on the stage. Without doubt the laugh springs from unconscious sympathy with the vigorous expletive. The theory easily fits also that large field of the comic which owes its power to the sudden humiliation and confounding of pride, pretension, formality, dignity, assumption of piety, learning or virtue, and the like. We are subconsciously on the side of the child or simple, unconventional individual who blurts out the plain, honest truth.

Beyond this lies a very wide region whose exploration exceeds the purpose of this essay, yet one or two ventures beyond the easy and obvious application of our theory will serve to show its unsuspected scope and adaptability. Take, for instance, the story cited by Bergson, and quoted by Dr. Sidis in his *Psychology of Laughter*. The anecdote relates to an M. P., who, when questioning the Home Secretary on the morrow of a terrible murder which took place in a railway carriage, remarks: "The assassin, after despatching his victim, must have got out the wrong side of the train, thereby infringing the Company's rules." Bergson explains the laughable effect of this remark by the implied automatic regulation of society, "an administrative regulation—setting itself up for a law of nature." Sidis dismisses it with the brief note—"There is nothing mechanical about it except the fact that the remark shows the stupidity of the M. P." But just where is the real crux of the story? Do we laugh at the implied automatism, or, on the other hand, at the assumed stupidity of the M. P.? Rather is not the smile caused by the sudden subordination of artificial, man-made rules by the

great, tragic, unconventional, and in a sense natural, act of murder? Shift the order in which the incidents are related, picture first the rigid regulations and then their sudden infraction by the escaping murderer, and we have tragedy, the direct predominance of the greater fact. But tune the imagination first to murder and then obtrude the petty rules as in any sense binding upon that wild, tragic,—even triumphant figure, and the wild, tragic and unconventional within ourselves exults in its superiority and priority,—and laughs.

Or, again, why do we laugh at the following anecdote related by Sidis? "An Irishman in a museum was looking at a copy of the Winged Victory, and asked an attendant what it was called, 'That is a statue of Victory, sir,' was the reply. Pat surveyed the headless and armless statue with renewed interest. 'Vichry, is it?' he said. 'Then begorry, Oi'd loike to see the other fellow.'"

Undoubtedly Pat was ignorant—of art, yet it is not this fact in itself which renders the story so delightfully refreshing. Spencer's theory of the cause of laughter as a descending congruity—the transference of consciousness from great things to small—is in order here; also the explanations offered by contrast, surprise, superiority, and Bergson's idea that we are amused at the intrusion of the body when the soul is meant. Yet these theories merely remove the problem a step farther from us. Why do we laugh at descending congruity, contrast, the body when the soul is meant? In attempting to apply our own explanation let it be noted that the story amuses in proportion to our appreciation of the value of the statue—as a work of art and as a symbol of man's highest victories. Is it that portion of ourselves in sympathy with the highest meaning of the work which laughs? Or is it that submerged nook where lurks approval of Pat's point of view,—which delights in his vision of a vanquished physical antagonist, minus even more than head and arms? Doubtless we harbor an unconscious pugnacity revolting in obscurity against the dominance of unfleshly ideals and which, through the medium of the story, is for the moment triumphant.

Passing finally to another variety of the comic, why do we laugh at the story of the Irishman who declared that the moon was of greater use than the sun because it shone at night, when without it we should be in darkness? The secret spring of amusement does not lie in the fact that the speaker is ignorant, for, state the matter in another manner,—say, "There is a man who does not know that the light of day

comes from the sun and that the moon's radiance is but a reflection from the same source," and we smile but slightly. Evidently the mirth-provoking element is to be found in the manner of making the statement. We laugh at the first form because we perceive the fallacy of the statement instantaneously, without a conscious process of reasoning. We delight in mental agility, just as on a lower plane we delight in bodily agility which in turn is a substitute for still more primitive activities.

There remains to be explained the tendency to break forth into laughter on occasions of great gravity, solemnity, sorrow, and even tragedy. Constituted as we are it is evident that the entire personality can not long remain keyed to the highest pitch. There is always the possibility of dissociation, of the sudden breaking away of the natural man under the strain,—or perhaps more truly the severance of the tense, solemn mood, resulting in relief and satisfaction on a lower plane. The mirth of the two women witnesses of a murder, before mentioned, may be referred to subconscious cruelty, and this in turn—it is probable—to still deeper repressions and perversions.

Apparently the natural method would have been to investigate first the laughter of childhood, but if the view advanced in this essay approaches truth it was not the child who first laughed. Repression of impulses and conscious regulation of the life could begin only with the adult. In time the artificial standards would be imposed on the child and at length inhibition would become organized in the race—a habit of the nervous system. Laughter, beginning as an accidental physical convulsion following indirect or delayed satisfaction of repressed impulses, would likewise have become a habit of the body and it is not difficult to conjecture why in the child, that bundle of budding instincts and premature impulses laughter should follow the slightest stimulus. Later, as body and mind are increasingly exercised, furnishing an outlet for subconscious impulses, laughter becomes less.

In a recent interesting article H. Addington Bruce<sup>1</sup> describes the laughter of childhood and in consequence all laughter as "a means whereby nature provides a salutary outlet for surplus nervous energy." But it is doubtful if we can, with strict scientific accuracy, speak of nature as deliberately and directly implanting an instinct "for the performance of an important physiological function." Among the various responses of a developing organism to environment those prov-

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<sup>1</sup> *The Outlook*, August 9, 1913.

ing useful would persist as habits and eventually become organized as instincts. The habit of laughter developed and survived because it was useful. The man who laughed heartily was among those fittest to survive. Furthermore, "surplus nervous energy" is but another name for unused power which could arise, in the first instance, only through the restraint and repression of natural functions. Thus it will be seen that the theory of laughter as a means for the outlet of nervous energy itself requires explanation, and this explanation is afforded, logically and satisfactorily it appears to the writer, by the doctrine of the satisfaction of subconscious tendencies.

It may be objected that the view of human nature involved in this theory of the origin of laughter is degrading. From the standpoint of the idealist the assumption of a special faculty of the mind whose function it is to recognize the presence of a definite humorous quality in events and acts would better accord with man's dignity and worth. But the evidence favors no such assumption. On the other hand, those who delve most deeply into the abyss of human mentality testify to the complexity, contradiction, and inequality of the elements of mind. The recognition of truth is in the end never degrading but prophylactic and remedial. Perceiving the function of laughter-provoking agencies we shall be slow to condemn even the broadest and coarsest humor, for this, furnishing an indirect outlet for suppressed instincts, may be more beneficent than we know. Lacking this relief the impulses might seek satisfaction in forms much less innocent. Laughter is born of the exigencies of evolving humanity and it will be long before its joyful echoes die from the earth.

# THE FORM OF THE CURVE OF PRACTICE IN THE CASE OF ADDITION

By EDWARD L. THORNDIKE, Teachers College, Columbia University

Each of 670 college students added daily for seven days forty-eight examples like those printed below, using seven printed sheets differing in the examples, but equal in average difficulty.

7	4	9	2	3	7	2	7
8	2	3	9	7	4	6	5
6	3	6	6	8	8	3	2
5	2	6	3	5	3	8	7
6	9	5	7	3	3	3	5
5	5	8	2	2	7	9	8
3	8	4	9	8	5	9	6
5	7	8	6	3	7	5	4
7	5	3	3	9	8	2	8
7	5	2	8	8	5	5	3
—	—	—	—	—	—	—	—

Each individual kept score of the time required and of the errors made. The scores for time required were kept with ordinary watches and are probably not very exact and are probably occasionally afflicted with large errors (of a minute or more).

I show in Fig. 1 the practice curves for the 336 columns, for the average of the 7 who were initially slowest, the average of the 13 next slowest, the average of the 29 next slowest, and so on, with groups numbering 36, 42, 61, 64, 45, 42, 73, 45, 41, 56, 52, 22, 22 and 21. These practice curves with two exceptions show the straight slope found by the author in a similar experiment with 19 adult students. (The two exceptions are the curve for the two most rapid groups. In the most rapid group there is a falling off with practice.<sup>1</sup>)

<sup>1</sup> The measurements which this curve represents are for special reasons much less reliable than those for the initially slower groups; and its eccentricities should not be taken too seriously.

In the next most rapid there is zero improvement at first and then a very rapid rise. The results are thus notably different from the average curve found by Wells with hospital attendants, and from that found by the author with children in the fourth grade. In these two latter cases the curve rises rapidly at first and then more and more slowly.

I am inclined to explain the difference between the straight slope characteristic of college and graduate students, and the parabolic form characteristic of hospital employees and young

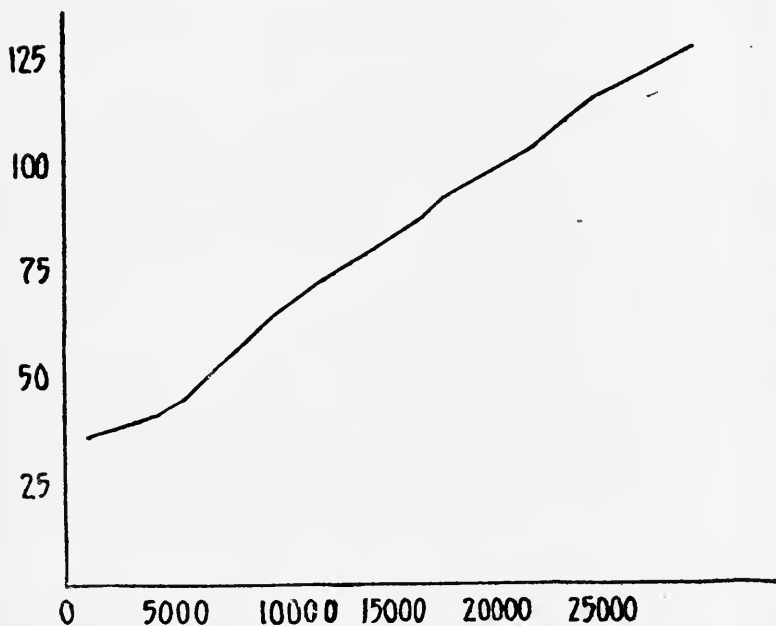


FIG. I.

school children as a result of differences in interest. This, however, is speculative. What is empirically shown is that the difference is *not* due to the fact that the college and graduate-student groups begin their practice at a much higher level. For the lower groups of these 670 students began at a lower ability than Wells' ten subjects; and, further, within the group there is no tendency for the initially lowest to show either a more rapid initial rise or greater negative acceleration than the initially higher group. If we should make a composite practice curve for improvement from 34 additions per 100 seconds to 150 additions per 100 seconds,



by taking the curve of the slowest group until it reached the height of the beginning of the next slowest group, then extending it by taking the curve of this next slowest group

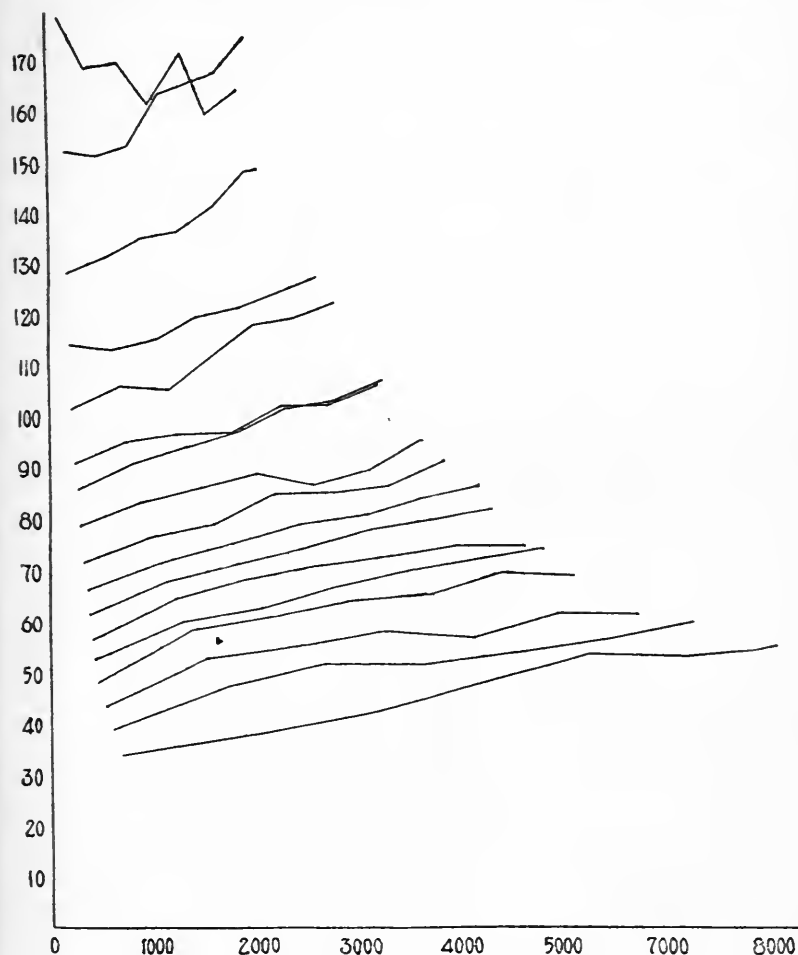


FIG. 2.

from its beginning until it reached the height of the beginning of the third group in order of slowness, and so on, the composite so obtained would not be at all of the parabolic

form. It would be very close to a straight line, as is shown in Fig. 2.

#### DESCRIPTION OF FIGURES 1 AND 2

Fig 1. Practice Curve in Single-Column Addition. Each curve represents the average of a number of individuals, the numbers being 7, 13, 29, 36, 42, 61, 64, 45, 42, 73, 45, 41, 56, 52, 22, 22 and 21 in order, beginning with the lowest curve. Distances along the base-line represent amounts of time devoted to practice. The vertical heights of the curve represent the number of additions per minute (writing the sum being counted as one addition).

Fig 2. Composite of All save the Two Highest Curves of Fig. 1. The lowest curve is taken from its beginning until it reaches the height of the beginning of the next lowest curve; then this next lowest curve is taken until it reaches the height of the second from the lowest curve; and so on.

## GHOSTS AND THE PROJECTION OF VISUAL IMAGES<sup>1</sup>

By LILLIEN J. MARTIN, Leland Stanford Junior University

Some tests recently made with three Stanford students who reported that they had seen apparitions show most clearly that whether we will see ghosts at all and what their appearance will be depends upon whether we normally project our visual images into space or are able to do so under the influence of a strong emotional stimulus. I give first, in the following, what the students gave to protocol in regard to the apparitions they had seen, and then briefly summarize the results of the tests I have made with them regarding their ability to project visual and other images.

I. Mr. M., special student in Chemistry.

"Time of appearance of the apparition—the first Sunday afternoon in May, 1908. Quite normal in health. I was reading with much interest in Mendeléeff's 'Principles of Chemistry.' That I was awake I am quite sure, as my sister had just passed through the room and we had spoken. Quite suddenly the book seemed to disappear, and in its place came the appearance of a man, head and shoulders. He wore a battered sombrero and soft dark shirt, open to the second button. About him was the seeming of brilliant sunlight and air of crystalline purity, and the sense of high places and much living out-of-doors. Though I could not see it, I knew that he carried a gun, probably a rifle, under his right arm, possibly suggested by the set of the shoulder. The face was attractive at first glance, rather handsome—or easily might have been. The skin was clear and beautifully bronzed. He looked back at me with a clear, open, frank gaze, with a half-smile on his face. But about him there was a swagger, a braggadocio, an insistence upon his own importance and his will to dominate without much consideration for the other fellow, that, though it was not consciously emphasized on his part, irritated me and left me somewhat ill at ease. He remained quite two minutes, while we stared at each other. The smile, somewhat superciliously amused, grew broader, and then slowly he faded away and the book blurred back into place. I finished the sentence and the paragraph.

Time, June 2nd, 1908, about midnight. I had been out to call, had particularly enjoyed the evening and had stayed late. I was sitting

<sup>1</sup> Martin:—Die Projektionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder. *Zeit. f. Psych.*, Bd. 61 (1912), p. 321, and published in book form by Barth, Leipzig.

on the edge of my bed unlacing my shoes. Suddenly I sensed another personality present and looked up quickly in surprise and inquiry. The same man stood across the room from me, perhaps ten feet away. The entire figure was visible. He was dressed much as before, but without the hat or the gun. If before his self-assurance had irritated me, there was now about him a conscious, purposeful, insistent masterfulness that I would not and could not endure. It aroused an instant and fiery antagonism. My feeling was—though I did not say it—"You get out of here, quick!" Yet I had a decided impression that he wanted something that I might give. But he did not ask, but demanded, in impossible ways. It seemed, too, that his manner was a pose, assumed as much to impose upon himself as upon me. A bit of unacknowledged fear lurked somewhere back in his consciousness. All this, somehow, I knew. He stared at me a moment, with his insolently irritating smile, took three steps to the side and diagonally toward me, and suddenly disappeared. The incident left me so irritated that I slept little. Early in the morning of June 3rd I left for the "High Sierra" on a two months' camping trip.

About three weeks later, when we reached the upper canyon of the Kern River, I had a very strong and quite unreasonable, persistent desire to go farther up the canyon, which was not possible at the time. This desire, after a day or so, associated itself with a strong sense of the personality of the man described above though I saw nothing. So compelling became the desire under the stimulus of this association that one night, somewhat after midnight, I walked almost without volition of my own, perhaps six miles up the trail in the moonlight until an impassible stream halted me. I seemed to *know* that this man was there up the canyon, and because he was there was I compelled to go.

We camped, three days after this experience, at the edge of Monachi Meadows. About midnight I was awakened, very suddenly, by someone shaking me roughly by the shoulder. I sat up. The same man stood beside me, hatless, but with his gun. I saw the glint of the moonlight on the gun barrel when he moved. He seemed to be in overwhelming terror. The qualities in him that had so irritated me before were quite gone,—the braggart was become the craven. I knew, somehow, that he came as a supplicant. He said nothing, nor did I. After staring at me a moment, his head drooped in sudden hopelessness, and he walked away.

On the first occasion I could not see the book through the man just described. The second time, he was transparent, so that a chifonier back of him was quite visible. So, too, the third time, I saw the trunk of a tree through him. That is, he was opaque the first time, but semi-transparent on the two other occasions.

In 1901, in connection with philosophy course, given by Dr. Lovejoy, I read much Hindu philosophy; and I remember that Dr. Lovejoy talked in his lectures of Egyptian religious beliefs, among others. I was much troubled that spring by a series of apparitions. I do not remember just when or under what circumstances these apparitions commenced, nor how many times they occurred, but they occurred very frequently. The details of each occasion of their appearance, what they did and said, I do not recall. One frequent astral visitor was a Hindu, seemingly a pundit or yogin; another, less frequent, was an ancient Egyptian, whom I much disliked. Upon the appearing of these apparitions I would be first conscious of the eyes, from which point the rest of the figures would slowly develop downward. Their

disappearing was the reverse process, from the feet to the eyes, which would persist sometimes much longer, at times for hours. The figures always were semi-transparent, though very distinct, so that objects were quite visible through them. They frequently talked to me. The Hindu, particularly, would deliver long discourses to me which at the time seemed the profoundest of wisdom, but which, upon later analysis, I always found to be a vague jumble of meaningless phrases. At first I permitted these apparitions because they amused me, and I could banish them at will. But later they became more persistent and I could not rid myself of them. Particularly the eyes would appear and follow me, without the rest of the figure becoming visible, which caused me much annoyance and distress. I remember quite distinctly the last occasion on which I saw them. I was alone in the country. Upon arising in the morning about 7:30 I went to the piano to play. Suddenly I saw the eyes of the Hindu above the piano. Of what happened thereafter I know nothing. I found myself at 9:30 in the evening exceedingly weary, still sitting upright at the piano. I do not know whether this was normal sleep or of a hypnotic nature. I had had some slight experience with auto-hypnosis, having on several occasions put myself to sleep by looking at a bright object, first resolving to awake in five or ten minutes.<sup>2</sup> I was so frightened by this experience at the piano that I thereupon determined never to see these particular apparitions, or their eyes, again, nor to go to sleep without my own volition, nor have I. I have, however, occasionally seen other eyes. For instance, a few evenings ago I was reading in bed when a single eye, about four inches long, appeared back and at the right side of my book. I glanced at it and continued my reading. Presently the eye came nearer and slid over the edge of the book. It was transparent, and the words were quite visible through it. It persisted about ten minutes. In general in looking at persons I am quite conscious of their eyes, as the eyes are to me the most noticeable and important feature of the face; just as the head is of greater importance than the rest of the figure to me. In looking back at this whole experience I see clearly that it is all to be explained by the projection of visual and other images, perhaps with auto-hypnosis as a subordinate factor.

As a child, from my earliest recollections to the age of about twelve, I had a considerable number of imaginary playmates who seemed very real to me and of whom I was very fond, so that I cared little to associate with other children. To each of them I gave a name, and each was a distinct and well characterized personality. When they finally ceased one by one to come to see me I often regretted their absence and wondered what had become of them. It was therefore, a matter of great surprise, in 1905, to meet on the street in Palo Alto, a young man (Dr. A. L. M $\ddot{u}$ nger, Jr.) whom at once I recognized as Futoni, one of those friends of my childhood. The fancy still persists that the two are identical. And I have since met two other men who just as surely are identical with my imaginary friends. These expe-

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<sup>2</sup> We see from this experience of Mr. M. that with him as a reagent we could doubtless easily have developed, artificially, at that time, a state of "Trance," and thus have been able to study the origin and evolution of a "medium." It would not have been necessary at the time of this occurrence to build up the "controls" by means of suggestion for they were already at hand in the persons of the Egyptian and Hindu.

riences of my childhood made me immediately accept as self-evident the theosophical idea of reincarnation when it was by chance presented to me in later years. That this particular group of apparitions were projected visual and other images, I am satisfied. As to their character, I am still in doubt, for my early environment does not seem to me to adequately account for their content."

I add still one more of Mr. M.'s projected visual images. "After spending the afternoon of Wednesday, March 18, 1914, in the petrography laboratory working at the same microscope with Mr. H., I called upon Mr. W. in Palo Alto. As Mr. W. and I were walking to the street-car about six o'clock, I saw a transparent image of Mr. H. walking just in front of Mr. W. on my left. When I left Mr. W. the image of Mr. H. followed me aboard the car, sat by my side, and went home with me. I confess I had a strong impulse formally to present him to my wife. The image sat in a chair in the living room when we went in to dinner. He was in the same chair after dinner, remaining with the family for half an hour. When I went upstairs to read the image followed me and remained until nine o'clock, when he walked out of the room."

Tests with Mr. M. in the laboratory, show: (1) That his visual images are strong and that when he casually thinks of an object such images are located in the back of his head, but if he thinks of the object in detail the image of it is out in front of him, the distance away depending upon the circumstances under which the image is seen. (2) When he projects visual images of persons he has seen, they are translucent. He does not usually see through such images but can easily do so. The transparency of his hunter, etc., images, was doubtless due to the presence of the bright light which was so emphasized as to divide the attention as, for example, in the case of seeing of the "glint" of the moonlight on the sun. (3) When he thinks in the laboratory in detail of the hunter, the projected visual image which appears has the same general appearance as the corresponding apparition except that it is slightly less transparent. The projected image in the laboratory seems "hardly as real" as his apparition, however. He feels it is an image of his apparition, that is, that it seems to have the same relation to it as does the projected image of an object. The emotional experience with its kinaesthetic accompaniments localized largely in the back of his neck and arms (he has a very slight lesion in the back of his neck which produces at times, a feeling of numbness and a dull ache in his arms), which are elements of the feeling of reality, are there but they are much decreased in strength. This hallucinatory experience as well as others like it show that it is not alone necessary to be able to project one's visual images and to do it usually or very frequently in order to have a hallucination. Mr. M. does on occasions when he has no hallucinations.

Other persons who have strong visual images and usually project them have never seen an apparition. The hallucination image must have a content or be accompanied by an emotion which separates it out from other projected visual images. (4) Mr. M.'s auditory images are projected. When he thinks of a sound he hears it again and it seems to come from where it originally came. (5) He can project touch images very imperfectly.

II. Miss T., a student in the German Department, graduated at Stanford University in December, 1913. Early in December I was told that Miss T. had seen a ghost. I give below a curtailed account of the interviews I had with her concerning the apparition she had seen.

December 8, 1913, evening. My own study. Miss T. said:—"I was awakened with a start on the night of December 5th, at 2:15 a. m. When I opened my eyes I saw my ghost, which I had often seen before, standing in my window. He has a high forehead, very hollow eyes, which appear to be only black blotches. His eyebrows are very dark; nose large; very sunken cheeks, and a very prominent chin. He wears a long white flowing robe. His hands are thin and bony. I lay in bed and looked at him. He did not move. He had a long chain in his hand. My bed is next to the window, so by putting out my hand I could have touched him. Finally I sat up. When I did so, he vanished out on the porch (which is off of my room) and sat on a couch out there. I got up and turned on my light and I could still see him, just as plain as when it was dark. Then I took my eyes off him and looked in the mirror to see if I were awake. Then I looked back and my ghost was still sitting in the same position. I crossed my room and turned on another light. He followed me, keeping about three feet behind me. I looked for the feet of the ghost but could not see them, on account of the long robes. Then I went out in the hall into another room. He followed. While I was in this room he stood in the open door. When I came out he stepped out of the way and allowed me to pass. He followed me until I reached my door and then vanished."

"Were you afraid of him?" "No. He fascinates me. I never want him to leave."

"Why not?" "I have always felt he would bring me good luck."

"Why did he come at this time?" "I do not know. But about five days before, our housemother had remarked that it was about time for my ghost to appear."

"Did he speak?" "No."

"How do you explain this ghost?" "I have always explained this as an hallucination, yet I have a feeling that it is a spirit and have always felt this. I feel that it comes from another world, of which we know nothing."

"What do you mean by an hallucination?" "It is a visual image so firmly fixed in one's mind, that it appears without any conscious effort on the part of the person."

"Did the ghost bring any message?" "No."

"Has it ever brought a message?" "No."

*Room was darkened.* "Can you see the ghost now?" "No. But I saw a large rectangle of white."

Second trial. "Even feeling that it would be a good omen, didn't make it come." (She has just written me that she has after many trials succeeded in getting a visual image of the ghost but only with closed eyes.)

"Give an account of previous experiences with this apparition." "It first appeared when I was about twelve years old. It was my idea of a ghost, from stories. It has appeared since on the average of twice a year. The apparition always appears in the window and grows more distinct each time. The other night it was closer to me than it ever had been, except in one instance. On this occasion, it came and sat on my bed, and was about to speak to me, when I called my roommate. The room was dark. The minute I spoke it vanished. I am sorry I called because I am sure it would have spoken to me. This ghost is always the same in appearance but always carries something different. I remember that once it had a large red book, another time an umbrella, and another time a key."

"Have you ever had any other similar experiences?" "No."

"Did you read ghost stories as a child?" "Only a few. My mother did not approve of my reading them."

"Was there anyone who encouraged you to read them?" "My aunt occasionally told me ghost stories."

"Is there any one in your family who believes in ghosts?" "No."

"What is your physical and mental condition in general when the ghost appears?" "It comes at times when I am physically well, happy, and not mentally tired."

Tests:—Instructed her to recall in turn, Dr. Jordan, Miss McCracken, etc., and to see them out in the room before her. She reported that she saw each person distinctly.

"Do they appear solid?" "Yes."

"Can you see Mrs. Herd?" "No, I don't know her very well."

"Can you put a chair at the side of this real chair?" "Yes: It is almost as real as the chair itself."

Pointing at a reproduction of Raphael's Sibyls, hanging on the wall, "Can you place at the side of that picture another like it?" "In general composition, yes, but not in detail." Repeated with Böcklin's Selbstportrait. "I can get Böcklin and the frame and mat clearly. The frame, however, is not as clear as the chair was."

Miss T. remarked during this visit that she was going to question the ghost next time. I suspect it will reply for trial showed she has the ability to project auditory images. Her images of touch are also projectable. She says this ghost is a proof or confirmation of what all her reading has more and more convinced her, namely, that there must be another world. Recently Miss T. has sent me some notes found in her diary in regard to the ghost. February 2, 1901:—"My dear old ghost was here last night. He had a Latin Grammar with him. I hope he doesn't have to struggle as I do with the subjunctive." July 15, 1901:—"The idea of carrying an umbrella in the middle of summer! That's all the sense my ghost has. He came bouncing in with an umbrella last night. He closed it as he entered through my window. He was so busy seeing if he could find any holes in my stockings, that he forgot to take his umbrella with him when he left. I couldn't find it any place in my room this morning, though I looked everywhere. I guess he came back and got it when I was asleep." January 9, 1902:—"My old ghost amuses me so. Last night, he did a fancy dance at my window. Then he came and rested in my rocking chair. The nerve of him! As soon as I spoke he vanished."



In my office two days after the tests mentioned above when I repeated some of them with like results. Miss T. gave me her reasons for not believing the ghost is merely a projected visual image:—1. The fact that the ghost came when she was well made her feel it was a real ghost for she supposed projected images came when one was tired or not well. 2. The fact that she was not afraid of it also made her feel it was real. 3. She cannot project her ghost at will or make it come at night when she wished. 4. She said, after I had tested her regarding her ability to project her visual images, that she had thought in the past that the ghost was more real than a projected visual image but recent trial with projecting the image of a very intimate friend had convinced her that the ghost was no more real than the image of the friend. She added that neither the ghost nor her friend's image follow her eyes in turning her head. The ghost, she also said, followed her when she walked away but her friend did not and then added, "she is very deliberate."

It will be seen from what has been just said, that Miss T. is ordinarily able not alone to project her visual but also her auditory and tactile images and yet she is not able to project her ghost at will. This fact shows that the ability to project one's images is but one of the factors involved in the seeing of ghosts. In the case of this ghost there is not only a favoring emotional factor which is connected with its appearing but one of an inhibitory character which interferes with its arising except on propitious occasions. The favoring and inhibiting emotional complex which has prevented a very lively visual image of childhood from disappearing is doubtless of a religious nature. The ghost coming, as its seer believes, from another world, is a source of intellectual comfort. Through its presence she has been enabled to successfully resist that skepticism regarding the existence of another world which she thinks might otherwise have been engendered by her University studies.

III. Mr. ———, student in the Department of Philosophy.

Lack of space prevents me from giving a detailed account of the numerous apparitions seen by Mr. ———. One observation I wish, however, to draw attention to as it is another confirmation of my opinion that ghosts arise only where one is able to project his images. Mr. ——— is able without difficulty to project his apparitions into space but he tells me that the images of his apparitions and the apparitions themselves communicate with him largely through signs. He very rarely hears them speak. Their preference for the gesture language was readily understood when it was found that all of his auditory images were very weak and that he is able only with great difficulty, if at all, to project them into space.

## SENSATION AND SYSTEM

By E. B. TITCHENER

Zeller remarked long ago that "the sole instruments of science are observation and logic," and Armstrong told us the other day that "true science rests wholly upon fact and upon logic."<sup>1</sup> These statements mean, I suppose, that in any system of science as we have it at any particular time two factors are distinguishable: the facts, the data of observation, which are obtained by the methods peculiar to the science in question; and the scaffolding of system, which is obtained by the logical method of reflection. The formulas err, no doubt, on the side of simplicity; but they are right in spirit, even if they need amendment in the letter. A system of science, considered apart from the facts which it embraces, is in reality a complex argument, and like all argument invites discussion.

In a recently published monograph<sup>2</sup> Rahn has passed in critical review certain of the current systems of psychology. He has devoted a good deal of space to an examination of my own attempts to bring our observational data into order. I am interested, of course, in what he has said, and I am grateful for the consideration that he has given me; I only wish that I could now take his criticism, so to say, on its merits, and (so far as I might be able) discuss his objections within a single perspective. That would be the natural procedure; but it is impossible for the reason that, while the monograph is dated December, 1913, the latest of my writings to which it makes reference is the *Text-book* of 1910.<sup>3</sup> There may easily have been circumstances, beyond the writer's control, that caused a delay in publication; but the delay is, so far as I am concerned, doubly unfortunate: first because I have published since 1910 several articles that bear upon questions of system; and secondly because a text-book (and the *Text-book* is most frequently cited) is written for the student and not for the professional psychologist, whereas a scientific article hopes to meet the critical eye. Unless, then, I am to say over again a good deal that has already been said in print, and am to spend time upon changes and corrections that are merely verbal, I

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<sup>1</sup> E. Zeller, in Prutz' *Deutsches Museum*, no. 11, 15 März, 1855; H. E. Armstrong, in *Nature*, Oct. 22, 1914, 215.

<sup>2</sup> C. Rahn, *The Relation of Sensation to Other Categories in Contemporary Psychology: a Study in the Psychology of Thinking*, *Psych. Rev. Mon. Suppl.*, xvi., Decr., 1913, no. 67.

<sup>3</sup> Nothing is said of a delay of publication in the author's Preface, and the list of References (128ff.) gives, curiously enough, only a few scattered dates.

cannot deal with Rahn's commentary as a whole.<sup>4</sup> I regret this limitation all the more since he writes in a sober and objective way.<sup>5</sup>

I begin—to make these remarks as coherent as may be—with a word about the sensation of classification and the sensation of observation. Sensation is a classificatory term. We get our first notion of it, probably, from a common-sense differentiation of the principal organs of sense; sensations come to us from the eye, from the ear, from the skin. We get our systematic notion of it, I believe, as the outcome of abstractive analyses performed under various psychological determinations; in other words,—while the common-sense notion, colored very likely by biology or physiology, is always with us,—we build up the notion of sensation, in a strict procedure, from observations of its empirical aspects or attributes.

Rahn, now, holds the opinion, which to me is strange, that the sensation of psychological classification may be observed (or, at any rate, ought to be observable), introspectively, all at once, in a single observation.<sup>6</sup> In point of fact, however, all experimental investigations of sensation deal with attributes: with qualities, intensities, durations, what not. Even the sensation of Wundt's system, which is constituted solely of quality and intensity, is to be observed under its

<sup>4</sup> Thus, I have discussed the psychological *process* (Rahn, 20-22) in this JOURNAL, xxiii., 1912, 497f.; the introspection of *clearness* (Rahn, 34f.) in *Journ. Phil. Psych. Sci. Meth.*, vii., 1910 (!), 180ff.; the psychology of *meaning* (Rahn, 48) in this JOURNAL, 1912, 165ff.; the psychological *purpose* (Rahn, 65) *ibid.*, 490ff. It is not to be expected that Rahn's criticisms and my own efforts at construction should tally point for point; but my attitude, *e. g.*, to his Section xviii. ("The Differentiation of 'Stimulus' and 'Meaning' Aspects of Percepts and Ideas") may readily be gathered from my paper in this JOURNAL.

<sup>5</sup> On one occasion too soberly! In *Feeling and Attention* (211) I quote Wundt on the distinction of intensity and clearness. "The citation from Wundt is correct; not so the interpretation that Titchener puts upon it. . . . In the very context from which Titchener takes the quotation, Wundt repudiates the interpretation that is put upon it" (Rahn, 17f.). But I supposed that everybody knew Wundt's doctrine; if anyone did not, I had stated it correctly earlier in *Feeling and Attention* (183: Rahn must have missed this passage); I showed by omission-marks that I was quoting only in part; and even so I could not avoid the fatal word *Eindruck*, which is not a Wundtian equivalent for *Empfindung*. What I meant as a mild literary joke—it is somewhat trying to have to explain!—has been taken by Rahn *au grand sérieux*.

There is another criticism which gives me, perhaps, some ground of complaint. I say that "the method of the physical and the psychological sciences is substantially the same" (*Text-book*, 24); and Rahn rebukes me on behalf of feeling (44; cf. 35). But I am not 'hedging' on that score; I expressly deal with the introspection of emotions on 472f.—a passage that Rahn has again missed, though it is separately indexed. I qualify because "the time is not yet ripe for a point-by-point comparison of the methods" (this JOURNAL, xxiii., 1912, 488; cf. the whole section, 485ff.).

<sup>6</sup> *Op. cit.*, 31, 55ff., 76ff., 106ff.

two attributes in separate experiments.<sup>7</sup> But be that as it may, I should certainly maintain in my own case that a sensation, taken in this way as a psychological object of the first order, must ordinarily, by the number and heterogeneity of its attributes, exceed what is called the 'range of attention.' Theoretically, there will be limiting cases: the one, in which the focal attribute is so exceedingly clear that the marginal attributes, correspondingly obscure, are observationally subliminal and touch off no report at all; the other, in which the main levels of consciousness are so nearly at one level that all the attributes touch off reports and all reports are practically alike in fullness of detail. I do not now discuss the question how nearly we are able, in the concrete case, to approach these limits. But I am sure that, in the very large majority of observations, some attribute or attributes will be definitely focal and others as definitely marginal.

It thus appears that Rahn's "anomalous case"<sup>8</sup> is, to my thinking, entirely normal. All observation of psychological objects of the first order is, I conceive, observation of attributes. If, then, we reduce consciousness to a single sensation,—as for the sake of argument we may be allowed to do,—and set to work to 'observe' this 'sensation,' we shall naturally (as I have just said) have some attribute, simple or compound, or some partial group of attributes, as focal, and other attributes as marginal. But this spreading out of a single sensation into coextension with consciousness, for the purposes of observation, does not mean that the sensation of our classificatory list should have two simultaneous clearnesses, a relatively high and a relatively low, or that clearness itself is an attribute of attributes and not an attribute of sensation. I am accustomed to think of the phase of excitation that corresponds with clearness of sensation as the release of a relatively constant amount of energy, the greater part of which flows freely in the course favored by determination, while the remainder flows less freely in the other, associated courses of the excitatory system. That may be mere figure of speech; but it helps us with our present puzzle. For the clearness that attaches to the sensation of our example is the total clearness of the consciousness which that sensation 'fills.' The current doctrine of attention, reinforced by common sense, leads us to emphasize the 'high light' and neglect the 'low light' of consciousness; and yet the total 'attention' that we are 'giving' at any moment is the sum of the two. The fact that every consciousness is patterned into relatively clear and relatively obscure is, to be sure, a fact which demands its own explanation. But it follows from this fact that, if a consciousness is composed of a single elementary process, some of the aspects of the process will be clear and others obscure; which clear and which obscure, is a matter of determination. All the clearness is, in this case, the clearness of the one process (as in more complex cases it is not); and the distribution of clearness, under a general law of consciousness, does not make it into two clearnesses. If, then, we may speak observationally of the 'clearness of a quality' and the

<sup>7</sup> Wundt makes the intensity and quality of pure sensation "jedes ein für sich bestehendes Object psychischer Messung," and bids us, in our search for the mental elements, look for irreducible and invariable 'quality': *Logik*, etc., ii., 2, 1895, 179f., 198. Yet it is the sensation, and not the intensity or quality, that is for Wundt the mental element.

<sup>8</sup> *Op. cit.*, 29.

'obscurity of an intensity' (and we might with the same right speak of the quality of an intensity, or of the quality or intensity of a particular degree of clearness), that manner of speaking does not at all commit us to a classification; the logical status of clearness, in relation to sensation, depends upon our rationalisation of all the observed facts. For, to repeat, the sensation of classification is the logical resultant of many observations. Its qualitative attributes are (in the typical case) points selected by definite procedure from a continuum; its intensive attributes are themselves continua, reduced by the same procedure to a series of points. Hence the conjunction of the qualitative attributes with any points whatsoever upon the correlated intensive scales constitutes, for classification, 'a' sensation; the bracketing together of the qualitative attributes with the complete intensive scales constitutes 'the' sensation.<sup>9</sup> It would be a great simplification of psychology if a sensation, *tota, teres, atque rotunda*, would stand before us under a single comprehensive determination and allow us adequately to observe it as a whole. But that, if it ever happens, happens only after we have made many separate observations of its distinguishable aspects.

Observation is, in fact, conditioned upon two things, peripheral reception and central disposition,—conditioned, as I have put it, upon excitation and determination. We group certain attributes together, in our classification, because, having observed one attribute under a certain determination, we thereafter invariably light upon certain other attributes when, with shift of determination but with excitation unchanged, we undertake to observe them. If, for instance, we have observed a hue, we can always and without exception, under the same conditions of excitation and with fitting determination in each case, observe a tint and a chroma; while we cannot by any kind or force of determination observe a sour or a warm.<sup>10</sup> We regard the attributes as constitutive, again, because the grouping or clustering is thus invariable; a hue that should present itself alone, that should confront us with a blank nothingness when we looked for tint or chroma, is unknown; where there is no observable tint or chroma, there is also no observable hue. In logical terms, the reduction to zero of any member of the attributive group destroys the whole process of sensation. And lastly, we give excitation the logical preference over determination, and take the 'sensation' rather than the 'attribute' as our mental element, because change of determination is limited in its effect to the unravelling of the groups or clusters already referred to; because, in other words, it shows us nothing more than the inseparable aspects of some simplest item of our subject-matter; whereas shift of excitation brings in new matter. We cannot

<sup>9</sup> I am not sure that Rahn is right in denying (as he seems to do: *op. cit.*, 27, 57) the intrinsic difference between qualitative and intensive attributes. In any event, the systematist is justified in making quality the basis of his classification, simply by the fact that qualities have received a goodly number of specific names. It would be absurd to throw away this logical advantage, even if the formal definitions of quality and intensity are (as to my knowledge they are not yet) reducible to identical terms.

<sup>10</sup> I have written elsewhere of psychological determination, and will here simply remind the reader that it need not always be definite, formulable in terms of the known, but may also be indefinite, a determination to look for 'something else of a qualitative sort' or even just to look for 'something different.'

pass, by mere shift of determination, from a hue to a sour; but by mere shift of excitation we may pass from hue to any other sensory quality that there may be. So the chemist (I take an instance at random from Jevons, though Rahn objects to chemical analogies) lists the alkaline metals as elements, and lists as their characters softness, fusibility, volatility at high temperatures, power to combine with oxygen, power to decompose water, and so forth. All analogies halt; but the point here is plain. The chemist, in observations of the first order, will always be experimenting upon softness or fusibility or some such character; and it is not likely that any single experiment will bring out all the characters at once. But he is still experimenting with a metal; the simplest item of his empirical subject-matter, the simplest object or phenomenon of his science, is in this particular chapter of his classification a metal. Hence metals figure as one group of the chemical elements, and the alkaline metals as a sub-group of the metals.<sup>11</sup>

Observation, however, is not confined to objects of the first order. Just as an ornithologist will identify, by species, sex and age, a bird that he has caught a mere glimpse of, on some excursion in the field, so may the psychological observer identify a mental process, elementary or complex, that he has previously made the subject of detailed attributive observation. When an observer who is discriminating tones reports 'kinaesthetic sensations,' the report does not mean that he can give, from his present experience, a precise description, qualitative, intensive, temporal, and so forth, of these sensations; it means simply that he has recognized certain items of experience, immediately, as 'kinaesthetic sensations,' in the way that the ornithologist recognizes his red-eyed vireo. From this point of view, the 'form' of a visual perception may, without any difficulty, become the observed 'content' of an experimental consciousness,<sup>12</sup> or a 'conscious attitude' may be reported as such; only, from the same point of view, we have no psychology of visual perception or of conscious attitude until we have analyzed it into its constituent processes and have brought these simpler processes under the terms of our ultimate classification. The difference between the common-sense and the psychological observation of (for example) visual form is in the first place a difference between the existential determination of science and the interpretative of everyday life, and secondly just this difference of ability or inability to reduce experience to its lowest observational terms.

Rahn makes a great deal, in this connection, of Külpe's experiments on abstraction. Let me remind the reader of the circumstances. In those experiments, a complex visual field was exposed for an eighth of a second. The field comprised four meaningless syllables, which were colored respectively red, green, violet, and black or grey. The syllables were composed each of two consonants and a vowel; no syllable was used twice in the experiment; and the vowels differed at any rate within a given field. The order of the colors, and the arrangement of the syllables themselves, varied from observation to

<sup>11</sup>Rahn, *op. cit.*, 25; W. S. Jevons, *The Principles of Science*, etc., 1900, 675.

<sup>12</sup>*Op. cit.*, 84ff., 89. I need not remind the reader that the nature of the datum of observation, in this instance, is in dispute. I am not here concerned, however, with 'form of combination' in any but an illustrative way.

observation. Five kinds of instruction were employed; the observer was directed (1) to note (*bestimmen*) the total number of visible letters (number), (2) to note the colors and their approximate position in the field (color), (3) to note the form which the syllables outlined (form), (4) to note as many individual letters as possible with their approximate position (elements), or (5) simply to observe. Külpe found—I take his extreme case—that under the instruction for form no report at all might be obtainable of color or elements. “The observers believed . . . that they had not, as a matter of fact, perceived any color, any object, etc.” From results of this sort far-reaching conclusions are drawn—as regards sensation! “Since psychology as science ascribes uniformly determinate attributes to sensations, regards them as constituted of determinate partial contents, it follows that psychology distinguishes between psychical processes and the consciousness of such processes.”

Here, then, is a highly complex visual ‘stimulus,’ presented tachistoscopically, and observed under cognitive instruction. For number, form, position and letter-element, whatever they may be, are certainly not sensations; and though the observers may have shifted to an existential attitude under the instruction for color, the probability is that they did not. Suppose, now, that under the instruction for form an observer has reported ‘diamond,’ and that he is able to say nothing when questioned regarding color and elements. What precisely, in the case of color, does this ‘nothing’ mean? It is hardly likely that the form was so bare, so disembodied, that it might have been, indifferently, a visual or a tactual form. We assume that it was perceived as a visual form, and might have been so reported. But then is it not reasonable to suppose, further, that it could have been reported as dark on light, or darker on lighter? For the stimulus is always projected upon the bright disc of the lantern-screen; a form that is made out at all must be made out upon this light background; can it be imagined that an observer, under the instruction for form, would be wholly unable to say whether the form glowed with a brighter light or stood out as darker upon the light surface? But dark and light are qualitative terms, and Külpe’s ‘color’ included black and grey. Let us turn, however, to the letter-elements. “The observer may be able correctly to describe a *form*, without having experienced anything whatever directly in *consciousness* in regard to the nature (*Beschaffenheit*) of the objects which delimited it.” What again, precisely, does the ‘nothing whatever’ mean? Under the instruction for elements, the observer is to note as many individual letters as possible. Does the ‘nothing whatever’ simply mean, then, that the observer could not say whether the form was bounded by individual letters? that he could not identify any part of the boundary as a letter? Apparently. But he might still have been able to report whether the boundary was continuous or discrete. mottled or uniform, broad or narrow, straight or wavy; more especially, one would think, since the report of contents not favored by instruction was given “vielfach nur auf Grund von Reproduktionen:” and adjectives of this kind do, in a way and to a degree, characterize the limiting objects.

Külpe’s account is tantalizingly brief. But we get an idea of the complexity of the field from the remark of one observer that he had frequently seen (or been conscious of) more than he was afterwards able to report. We get an idea of the cognitive emphasis in the instruction from the remark of another observer that the process of

noting or specifying (*bestimmen*) seemed to be entirely separate from that of seeing. Külpe's own inference that the changes wrought by instruction are changes of *Auffassung* and not of *Empfindung* seems imperilled by his later statement that "die Versuchspersonen glaubten tatsächlich die Eindrücke in der angegebenen Unbestimmtheit zu *sehen*" (italics in original). But indeed the word *Empfindung* is sadly out of place in this context. Here are apprehension, identification, meaning, perception, reproduction; here, in other words, is observation solely of the higher orders. To argue from the introspective reports—by way of sensation!—to a rehabilitation of the 'inner sense,' and a distinction between conscious actuality and psychical reality, and a furtherance of the old dispute between nominalism and realism, is to erect a heavy superstructure of system upon curiously insufficient observational foundations.<sup>13</sup>

What it is, in observations of a higher order, that touches off the introspective report is a matter to be discovered by experiment in the particular case. I have not repeated Külpe's experiments, and therefore cannot—in this particular case—be specific.<sup>14</sup> But I can now make a general statement which may be to Rahn's point. I spoke above of limiting cases in the observation of the sensation-consciousness, and I left them without discussion (p. 260). But I have no hesitation in saying that the realisation of the first limiting case has no terrors for me; and that certainly I should not be driven by it to a distinction of conscious actuality and psychical reality. There is no difficulty in conceiving an observer to be so completely under the influence of determination that the marginal attributes are too obscure to connect at all with the mechanism of report. Külpe remarks, and correctly, that "die Lücken im Protokoll erlauben nicht ohne weiteres den Schluss auf eine entsprechende Lücke im Bewusstsein der Versuchspersonen;" and it seems fair to suppose in our imaginary instance—just because that instance comes to us as a limiting case—that the margin was still 'there,' in consciousness, although it was so obscurely 'there' that it failed to touch off a verbal description. Might we not, from what we know of the pattern of consciousness, even predict, before the event, that once in a great while such cases will occur?

But the disagreement between Rahn and myself goes deeper, I am afraid, than this matter of classification, and of the relation of scientific system to scientific data. I cannot help thinking that he

<sup>13</sup> O. Külpe, *Versuche über Abstraktion*, Bericht über den I. Kongress für experimentelle Psychologie in Giessen, 1904, 56ff. Rahn even translates *Beschaffenheit* (of the letter-element) by 'attributes,' as if the letter-element were a sensation: *op. cit.*, 78.

<sup>14</sup> The instruction, as I have said, was cognitive; the observer was asked to note or determine or specify (*bestimmen*). It is fairly certain that, under this cognitive determination, something in the visual field, some phase of the complex 'stimulus,' actualized the 'meaning' of the instruction, which then touched off the report. The focus of consciousness would thus be held by processes correlated with the stimulus-phase and the meaning. It would probably not be easy, in the circumstances, to indicate all these processes; and it would probably be impossible to name their relevant attributes, and so to reduce the observation outright to that of an object of the first order. Külpe says (*op. cit.*, 66): "Die Durchführung der Aufgaben ist nicht näher zu beschreiben," 'cannot be described in detail.'



is still, as regards the psychology of sensation, held in the trammels of the older associationism. It would indeed be surprising, were it not so common, to note how the critic of current sensationalism persists in ascribing to sensation, almost as if by instinct, the characters which the associationist school attributed to idea. In the typical associationist system the idea was, first and foremost, a meaning; as a meaning, it was stable and permanent; and its association to other and similar ideas gave rise to all kinds of mental formation. We may insist, now, that meaning has been pared from sensation,—we may insist that meaning, from the psychological point of view, is in general something detachable, something that accrues, something variable and incidental; yet we cannot drive that point home; our critics still write as if sensation involved awareness. We may insist, again, that sensation is a process; we may give illustration, and try in all manner of ways to make clear what, in the psychological sense, a process is; we are told that the position is untenable and impossible, and that our sensory element must in reality be something fixed and static. We may insist that sensation is an analytical and not a genetic concept; and we are told, notwithstanding, that it is generative: not any more, perhaps, that it generates our perceptions,—that mistake one may hope never again to meet in such form that it need be taken seriously; but certainly that it ought to generate better than it does, that it is inadequate to our complexes. I say 'we insist,' because I am here outlining a position in dogmatic terms; in fact, of course, we argue from such evidence as we can bring forward. But the arguments and the evidence seem hardly to count; it is the position itself, if I am not mistaken, that is repugnant, that is instinctively rejected; a consideration of arguments and evidence—as distinct from a mere confrontation of system with system—would be very welcome. And the reason for the repugnance seems to lie in the carrying over into sensationalism of the doctrines of associationism.

Thus, at the very beginning of his enquiry, Rahn refers to "the structuralistic doctrine that under certain conditions we may become aware of redness and blueness."<sup>15</sup> At first thought one would say that this is a common-sense rather than a psychological doctrine. But the context shows that Rahn looks upon 'awareness of redness and blueness' as a paraphrase of the structural term 'color sensation.' As well might one declare that 'awareness of fusibility' is a particular chemical element! The phrase is, indeed, quite foreign to the structuralist's ways of thinking. He can 'observe a red,' and he can—like everybody else—'apprehend a meaning;' observation he will deal with under the heading of Method, and meaning he will deal with, from the point of view of analytical psychology, under its own heading. But he has nothing more to do with 'awareness' than has the chemist or the physicist or the biologist; and yet Rahn, without hesitation and in good faith, introduces the term into a passage that refers to structuralist doctrine! With this prepossession upon him, how shall the critic come to close quarters with the system he is criticizing?

As with meaning, so with process: "if the element is to be considered as a part, an irreducible ultimate content of consciousness, then it cannot at the same time be regarded as a process, for a

<sup>15</sup> *Op cit.*, 3; cf. 60, 62, 78. etc. I do not see how the charges brought against me on 40, 94, are to be squared with the sentence from which I quote; but they are themselves met, I hope, by *Thought-processes* 60f. and *Text-book* 545.

process implies the interrelation of a number of elements or factors;” and so the poor element, in spite of protest, must remain “static.”<sup>16</sup> Naturally, if sensation is a meaning and consciousness is awareness! But might it not have occurred to Rahn that things are less simple than this? that, if his process is the resultant of several elements and mine requires but a single element, we may perhaps not define ‘process’ in the same way? And might it not have been both more effective and more profitable to understand my ‘process’ and wrestle with that than to substitute another kind of process for it?

Here are two of our associationist parallels, plainly drawn. As for the third, Rahn is clear of the cruder form of what I must call—from my own standpoint—the genetic fallacy; he by no means escapes it altogether. The “kinaesthetic sensations that attention lights upon, when it pounces upon such a subtle psychosis as a state of doubt, are not the psychic equivalents of the state itself.”<sup>17</sup> But, it seems to me, the task of psychology, over against the ‘I doubt’ which expresses some common-sense or logical determination of the organism, is precisely the task of analysis; and if analysis brings to light nothing else than kinaesthetic sensations, then these sensations (characterised as accurately as may be) are precisely the ‘psychic equivalents’ of the logical situation. Description of process is thus correlated with statement of meaning. If Rahn now replies—as he probably would—that innumerable analyses by the most competent observers would still fail to put him into what he regards as psychological possession of ‘so subtle a psychosis,’ I must repeat what I have said before: first, that a description, as compared with an intimation of meaning, is always clumsy and long-winded; but, secondly, that psychology—as I regard it—“is under no obligation to reconstitute, at the end of an investigation, the [continuum of] psychical experience; it is enough to have shown that its contributions to the analysis of that experience are valid.”<sup>18</sup> In other words, as I have also said elsewhere, a descriptive science may not justly be asked to give more than, by its nature as descriptive science, it can properly furnish.

James Mill, although he tries hard to make his readers get the qualitative ‘feels’ of the various sensations, can nevertheless speak of sensations of resistance, of hardness, of weight, of shape, of size, because the sensations are, for him, “the feelings from which we derive our notions of what we denominate the external world;” “it is acknowledged on all hands that we know nothing of objects, but the sensations we have from them;” “in using the names tree, horse, man, the names of what I call objects, I am referring, and can be referring, only to my own sensations.”<sup>19</sup> Mill, therefore, is perfectly logical in regarding a process as an affair of “certain steps,” and in declaring that consciousness of change is “an essential part of the process” of sensation: “having *two* sensations is not only having sensation, but the only thing which can, in strictness, be called having sensation.”<sup>20</sup> And Mill can undertake to resolve doubt—“a

<sup>16</sup> *Op. cit.*, 21f., 67.

<sup>17</sup> *Op. cit.*, 45.

<sup>18</sup> This JOURNAL, xxiii., 1912, 166; *Thought-processes*, 1909, 284f.

<sup>19</sup> *Analysis of the Phenomena of the Human Mind*, i., [1820] 1869, 58, 77, 79; 3; 349f.; 93; etc., etc. I refer to sensation, rather than to the more obvious idea, in order to bring out clearly the resemblance between Rahn and a representative member of the associationist school.

<sup>20</sup> *Op. cit.*, ii., 13f.; 11f. The whole section is significant.

phenomenon of some complexity, but of which the elements are not very difficult to trace"<sup>21</sup>—into 'psychic equivalents' that shall leave no remainder, because he is translating common sense into associationist terms and offering the result as psychological analysis. All this is intelligible enough; but it is not structural psychology.

There will probably be no end to the rivalry of psychological systems. A science is never finished; and as the data of observation multiply, the systematic argument must be changed and extended to cover them. That apart, psychology is hardly yet free from its traditional connection with philosophy, and psychologists are accordingly not yet at one even as regards the general type to which a system must conform. No more can be said, therefore, than that here, as everywhere, the system is subordinate to the data, and reflection secondary to observation. But it is well to keep this principle steadily in mind: for only in its light shall we apply the right test to our systems, the test of their ability to organise, consistently and exhaustively, such data as we have. If we thus become a little mistrustful of programmes, is there any harm?

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<sup>21</sup> *Op. cit.*, i., 270, 430.

## SOUND LOCALIZATION UNDER DETERMINED EXPECTATION<sup>1</sup>

By L. R. GEISSLER

It had frequently been observed by the writer that under ordinary circumstances his sound-localization seemed to be strongly influenced by a determined expectation or direction of attention. The only direct reference to this problem in the experimental literature known to the author is that made by Pierce,<sup>2</sup> in connection with experiments in which "the method of procedure was to move the sound until the subject was aware of the direction in which movement was taking place." In discussing the results, Pierce says: "from among the various directions theoretically possible from a physical point of view, certain circumstances will usually determine which direction shall be actually perceived. The expectation of the subject is often the deciding factor. This expectation may be the result of a recent localization which makes one direction of the attention easier than another, or it may be due to some suggestion inadvertently given. And when once a given direction of movement has been perceived, false though it may be, localizations of adjacent positions will the more readily fall in line with the direction first perceived. This determining influence of both suggestion and of an antecedent localization will appear still more clearly on later pages." Our present experiments were planned for the special purpose of determining quantitatively to what extent the expectation of a stationary sound from a certain direction influenced the localization of this sound.

The writer was greatly aided in the conduct of these experiments by Mrs. Ruth Collins Desch, who had previously in co-operation with C. E. Ferree investigated another phase of the problem of auditory localization.<sup>3</sup> Our experiments began Jan. 24th and lasted to April 25th, 1914. They were performed on twenty observers, each of whom made 68 individual localizations which could be finished in a single session of not more than about 90 minutes. Twelve observers were men students of the senior class who had completed a full year's introductory course (based on Titchener's "Textbook"), and about four months or more of qualitative laboratory work (based on Titchener's Qualitative Students' Manual), which had included in every case the experiment on touch-localization (No. 33) as a sort of special preliminary training. The eight women observers ranked from the junior class of a girls' preparatory school to teachers of several years' experience in that school; none of them had had special

<sup>1</sup> From the Psychological Laboratory of the University of Georgia, Athens, Ga. Read in parts before the Am. Psych. Association, Dec. 31, 1914, Philadelphia, Pa.

<sup>2</sup> A. H. Pierce, *Studies in Auditory and Visual Space Perception*, 1901, 57.

<sup>3</sup> C. E. Ferree and Ruth Collins, *An Experimental Demonstration of the Binaural Ratio as a Factor in Auditory Localization*, *Am. J. Ps.*, April 1911, xxii, 250-297.

psychological preparation. The writer gladly takes this opportunity to express his hearty thanks to Mrs. Desch for many helpful suggestions in performing the experiments and in preparing this article, as well as to Misses M. E. Jeffcott, Dorothy Selby, Sara Lamar, Helen Michael, Janet Newton, Pauline Odum, Julia Orr, Ruth Reid, and to Messrs. G. Barrett, A. B. Berndt, T. N. Hendricks, W. E. Hitchcock, F. Holden, T. A. Maxwell, J. E. McDonald, L. Michael, J. Myers, H. M. Pitts, E. R. Pund, and H. D. Williams, for their patient and conscientious work as observers and their deep interest in the problem.

The sound-cage was built in the middle of a symmetrically constructed room of  $3.1 \times 6.2$  meters, which contained only one table and two chairs, so that possible echoes from the faint stimulus were neutralized or, more probably, eliminated. The observer's head, facing the windows, was in the center of a noiselessly rotating wooden lever-system which carried two little telephone receivers at exactly opposite positions, about 75 cm. from the center of the head, and at the level of the ears when the observer sat on a chair placed in the center of the apparatus. The sound-stimulus was produced by noiselessly and quickly closing and opening a double-throw baby-knife switch in circuit with the receiver and two dry cells. The observers were blindfolded, care being taken that the bandage did not touch the pinna. The position of the stimulus was determined by means of a protractor-scale in the center of the lever-system above the observer's head. The zero-point was at the back, 180 in front, the positions on the right were called *plus*, those on the left *minus*, so that the aural axis passed through  $\pm 90^\circ$ . While the sound could be given at any point on the circumference of the horizontal plane passing through the ears, the observers were instructed that it would be given only at  $10^\circ$  intervals; actually it was given only at intervals of  $30^\circ$ , namely 0,  $\pm 30$ ,  $\pm 60$ ,  $\pm 90$ ,  $\pm 120$ ,  $\pm 150$ , and 180. The observers were given a sufficient number of preliminary trials to memorize this schema. The localizations were made in two ways, by pointing with finger and by visual-verbal reference to the scale, unless the latter method was preferred exclusively. The bandaged eyes prevented the use of the cardboard chart employed by some investigators. Whenever an observer used both methods, care was taken that pointing and verbal reference to scale did not conflict.

In our method the greatest importance must be laid on the instructions given to the observers and the subsequent reports demanded from them. After having been told that the general purpose of the investigation was to find out how well they could localize a faint sound under certain circumstances, they were instructed sometimes to direct their best attention to one part of the circle only, to think of it and nothing else, and to expect the sound to come from that particular direction. If sometimes the sound seemed to come from a place entirely different from the one indicated, that fact should not in any way disturb their future observations, as such differences were naturally to be expected. Again, they were told that sometimes they were not to limit their attention and expectation, not to expect the sound from any definite point; this instruction was briefly expressed by the command "Unlimited." Then, as soon as the sound was heard, its position was to be indicated immediately, and in addition the observers were asked to tell from what point they had expected the sound, either definitely or in general, that is, whether from points or from areas or regions on the scale. They were urged to be perfectly frank to tell whether they had disobeyed the instruction or not, as that mistake was less serious than deception. Aside from the

behavior toward the instruction "Unlimited," which will be dealt with in detail under the rubric of results, several observers reported failures to follow the instruction for special direction of expectation in certain cases, in which the experiments were repeated at a later time. Both reports of localization and expectation were recorded, and the writer is confident that instructions were conscientiously carried out by all observers. In addition to these reports, ten of our men observers were also asked at the end of their respective sessions to give an introspective account of the usual course of consciousness during an observation, beginning with the instruction and stopping with the beginning of the report.

The 68 observations made by each subject were divided into two groups, the first containing 5 series of ten tests each. A short pause was made after each of these series and a longer one after the whole group. The 18 observations of the second group were taken without any interruptions. In the first group, before each observation, the subject was instructed whether to limit his expectation of the sound to one of the halves, by the words "right half," "left half," "front half," or "back half," or whether not to expect the sound from anywhere in particular, by the word "unlimited." A second after this instruction the warning signal "ready" was given, and after about two more seconds the sound-stimulus was presented. The positions 0 and 180 were never used with the instruction "right" or "left;" likewise  $\pm 90$  were omitted as stimuli when expectation was to be limited to the front or back half. Each of the 12 stimulus-positions was presented once with the instruction "unlimited." With limited expectation the stimulus was presented once at each position in the *required* half and once at each position in the *opposite* half. The intermediate positions  $\pm 30$ ,  $\pm 60$ ,  $\pm 120$ , and  $\pm 150$  had thus to occur four times each, while the corner positions  $\pm 90$ , 0, and 180 occurred only twice each, in addition to the single occurrence with "unlimited" as instruction. Each series of ten observations included two "unlimited" cases and two cases for each half, in such a way that the stimulus was actually given in the *same* half as often as in the *opposite* half. In the second group of experiments the expectation was limited to the quadrants by the instructions "right front," "left front," "right back," and "left back." Only the intermediate positions were used as stimuli, except in one "unlimited" case and in another case where the sound was presented in some "different" position; otherwise these experiments were carried on like those of the first group. The following schema represents the actual order of instructions and corresponding stimulus-positions which each of the twenty observers completed.

SCHEMA OF ORDER OF PROCEDURE

Instr.	Stim. Pos.	Ser. 1	Ser. 2	Ser. 3	Ser. 4	Ser. 5	Instr.	Stim. Pos.	Quadrants
Unlim't'd	anywhere	+120	+90	+30	+150	+60	Rt. Fr.	same Qu.	+150   +120
Fr. half	same half	-120	180	-150	+120	+150	L. B'k.	diff. "	+30   +150
Rt. "	opp. "	-90	-150	-120	-30	-60	L. Fr.	same "	-120   -150
L. "	same "	-30	-60	-90	-120	-150	Rt. B'k.	diff. "	180   -120
B'k. "	opp. "	180	+150	+120	-150	-120	Unlim.	anywh.	-60   0
Rt. "	same "	+150	+120	+90	+60	+30	L. B'k.	same Qu.	-30   -60
Unlim't'd	anywhere	-150	-120	-30	180	-90	Rt. Fr.	diff. "	-150   -30
Fr. half	opp. half	+30	+60	0	-60	-30	Rt. B'k.	same "	+60   +30
B'k. "	same "	-60	-30	+60	+30	0	L. Fr.	diff. "	+120   +60
L. "	opp. "	+60	+30	+150	+90	+120			

A brief examination of this schema will show that the same combination of instruction and stimulus-position was never given more than once in each series.

In computing and tabulating our results we fractionated the data both as to stimulus-position and as to instruction concerning the direction of expectation, and averaged either localizations of all twenty observers or of one group of observations as compared with another group. The percentages of error in localization for any given direction of expectation with all possible stimulus-positions were calculated by adding the errors, that is, the differences between the stimulus-positions and the corresponding averages, and dividing this sum by the sum of all employed stimulus-positions. Conversely, the magnitude of error, or amount of displacement, for any given stimulus-position under all kinds of expectation employed was determined by adding the corresponding averages, dividing the sum by the number of cases involved, and subtracting the result from the stimulus-position. In the Tables which follow the decimal fractions have been omitted, although at least one decimal position was always used in the calculations. Among the various abbreviations used in the Tables, No. refers always to the number of cases from which the average or error is calculated. MV. stands for mean-variation, E. means error, and Tot. or T. stands for total; other abbreviations will be self-evident or explained in the subsequent discussion of the Table in which they are used.

Before attempting to determine whether direction of expectation has any influence upon sound-localization, it is necessary to study our data from a general standpoint and as far as possible to compare them with results obtained by previous workers in this field. For this purpose we must refer the reader to rubric h of Table I and to the first 28 rows of Table II. These results show clearly the general tendency of sound-displacement that other authors have found, namely that sounds from behind the ears are frequently referred forward and those from in front are referred backward, that is, are referred toward the aural axis or in some cases even beyond it. In our results the greatest displacement occurs with the stimulus at 0, the sixty localizations averaging 129.2; but if we subtract from these the twenty localizations under instructed forward direction of expectation, the remaining forty observations average still as high as 123.2. The actual places at which stimulus 0 was localized are shown in rows 1 and 2 of Table II. In row 2, for example, that is with "unlimited" and "back half" direction of expectation, the sound coming from 0 was localized 9 times at 0, twice at 10, once at 120, 3 times at 150, and so forth. Likewise all the other even-numbered rows contain the reduced observations, that is, those localizations in which the direction of expectation was to the same half in which the sound would be given. With the exception of stimulus-positions  $\pm 90$  the "reduced" observations show a smaller displacement error than the total, thus indicating in general the unfavorable influence of direction of expectation upon the accuracy of sound-localization. With the stimulus at 180 our results show a good deal less backward displacement than other investigators seem to have observed, the error being only 10.5 for the reduced number of observations. The smallest errors occur with stimuli 90, which show a slight backward displacement of about 5°. The second largest error is found with 30, a displacement of 75.2° and 67.5° for the right and left side respectively, while all other stimuli show an error varying between 19.0° and 25.0°, either forward or backward.

TABLE I

Rows	Rubrics	a			b			c			d			e		
	Instr. Stimulus Position	Unlimited or No Expectation									Expectation Limited to Right or Left					
		Gen. Results			No Exp'n.			Vol. Lim. Exp'n.			St. in Opp. H.			St. in Same H.		
		Av. Mv.	E.		No. Av.	Mv.		No. Av.	Mv.		Av. Mv.	E.		Av. Mv.	E.	
1	0	135	54	135	3	120	40	17	138	49						
2	+30	120	23	90	7	121	30	13	119	20	109	37	79	119	26	89
3	-30	116	53	86	5	66	47	15	133	32	104	38	74	102	54	72
4	+60	87	15	27	6	108	18	14	81	14	84	14	24	92	8	32
5	-60	91	16	31	7	86	13	13	98	11	88	14	28	85	20	25
6	Tot. Bck.	110	..	74	28	100	..	72	114	..	94	..	51	100	..	55
7	+90	86	12	4	6	95	13	14	83	10	91	5	1	82	8	8
8	-90	90	13	0	5	84	9	15	93	15	88	12	2	89	12	1
9	+120	91	12	29	9	93	9	11	90	14	95	15	25	95	22	25
10	-120	107	15	13	8	109	21	12	106	12	99	10	21	91	8	21
11	+150	130	16	20	5	126	19	15	131	20	137	25	13	115	23	35
12	-150	139	13	11	8	120	24	12	152	19	142	12	8	125	23	25
13	180	151	29	29	6	142	57	14	155	39	..	..	..	..	..	..
14	Tot. Fr.	124	..	20	36	119	..	64	127	..	118	..	17	107	..	28
15	No. Expt.	240			75			165			200			200		
16	ΣErrors	475			462			487			275			331		
	Σ St. Pos.	1080			1080			1080			900			900		
		=44.0%			=42.8%			=45.2%			=30.0%			=36.8%		
17	Eliminating 0, ±90, and 180	307			293			316			272			323		
		720			720			720			720			720		
		=42.6%			=40.7%			=44.0%			=37.8%			=44.9%		



TABLE I—*Continued*

Rows	Rubrics	f			g			h			i			j		
	Instr. Stimulus Position	Expectation Lim. to Front or Back						Total Results Rubrics a and d-g			Expectation Lim. to Quadrants					
		In opp. half			In same half						St. in diff.			In same Qu.		
		Av.	Mv.	E.	Av.	Mv.	E.	No.	Av.	E.	Av.	Mv.	E.	Av.	Mv.	E.
1	0	141	56	141	110	75	110	60	129	129	110	44	80	85	33	55
2	+30	126	88	96	76	44	46	100	109	79	110	44	80	85	33	55
3	-30	117	46	87	74	53	44	100	103	73	126	32	96	85	45	55
4	+60	99	13	39	74	12	44	100	87	27	98	15	38	66	14	6
5	-60	93	11	33	76	18	16	100	87	27	Not given			72	14	12
6	Tot. Back	115	..	79	82	..	46	460	103	67	111	..	71	77	..	32
7	+90	..	..	..	..	..	..	60	86	4	..	..	..	..	..	..
8	-90	..	..	..	..	..	..	60	86	4	..	..	..	..	..	..
9	+120	82	22	38	104	10	16	100	94	26	103	11	17	102	11	18
10	-120	87	11	33	99	10	21	100	97	23	102	12	18	96	12	24
11	+150	113	37	37	126	20	24	100	123	27	138	18	12	140	12	10
12	-150	100	43	50	139	16	11	100	129	21	140	11	10	132	14	18
13	180	106	81	74	167	26	13	60	142	38	160	15	20	..	..	..
14	Tot. Front	98	..	46	127	..	17	460	115	29	129	..	15	118	..	20
15	No. Expt.	200			200			1040			160			160		
16	$\Sigma$ Errors	630			294			570			290			208		
	$\Sigma$ St. Pos.	900			900			1080			840			720		
		=70.0%			=32.7%			=47.0%			=36.0%			=29.0%		
17	Eliminating 0, $\pm 90$ , and 180	415			171			295			270			208		
		720			720			720			660			720		
		=57.6%			=23.8%			=40.1%			=40.9%			=29.0%		

TABLE II

Possible Positions:		0	10	20	30	40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	Tot. No.	Av.	Error
1	Tot.	13	2	2	2	2	2	2	2	2	2	2	2	2	1	2	3	6	10	25	60	129.2	129.2
2	Red.	9	2	2	2	2	2	2	2	2	2	2	2	2	1	2	3	5	6	14	40	123.2	123.2
3	Tot.	..	1	4	2	6	2	5	2	3	7	8	6	17	8	5	9	7	4	4	100	109.1	79.1
4	Red.	..	1	3	2	3	2	5	2	2	4	6	3	9	6	4	7	2	2	6	60	105.2	75.2
5	Tot.	4	4	3	1	1	2	2	1	1	3	3	3	10	3	1	6	16	8	6	100	102.7	72.7
6	Red.	4	4	3	5	4	1	2	1	1	1	1	1	6	1	1	6	10	5	3	60	97.5	67.5
7	Tot.	1	..	..	..	2	2	11	5	12	41	14	4	5	2	2	1	..	..	..	100	86.9	26.9
8	Red.	1	..	..	..	1	1	8	12	9	23	9	3	3	3	3	1	..	..	..	60	83.5	23.5
9	Tot.	..	1	..	1	2	1	11	12	8	39	9	6	3	2	3	1	..	..	..	100	86.9	26.9
10	Red.	..	..	..	1	2	1	9	9	5	21	3	2	1	2	3	1	..	..	..	60	84.8	24.8
11	Tot.	..	..	..	..	1	..	5	2	12	31	6	3	2	..	..	..	..	..	..	60	86.0	4.0
12	Red.	..	..	..	..	1	..	4	2	8	20	3	2	3	3	1	..	..	..	..	40	84.5	5.5
13	Tot.	..	..	1	..	2	2	3	4	7	29	6	3	3	3	1	..	..	..	1	60	86.1	3.9
14	Red.	..	..	..	..	..	1	3	4	5	18	4	1	2	1	..	..	..	..	1	40	85.2	4.8
15	Tot.	1	..	..	4	1	1	1	2	9	33	22	14	8	2	1	..	1	..	..	100	93.6	26.4
16	Red.	..	..	..	1	1	..	..	2	4	20	15	10	5	1	1	..	..	..	..	60	96.7	23.3
17	Tot.	..	..	..	1	..	..	..	5	7	41	22	15	5	1	1	1	..	..	1	100	96.8	23.2
18	Red.	..	..	..	..	..	..	..	2	6	20	15	10	4	1	1	..	..	..	1	60	99.2	20.8
19	Tot.	1	1	..	2	1	3	..	2	3	3	6	8	18	11	5	19	14	2	1	100	122.9	27.1
20	Red.	..	1	1	1	..	2	..	1	1	1	3	7	14	6	3	13	8	5	..	60	125.0	25.0
21	Tot.	1	2	2	2	..	1	1	1	4	7	6	2	9	17	15	15	13	5	..	100	129.3	20.7
22	Red.	..	1	1	..	..	..	..	..	1	2	6	2	9	9	12	7	8	3	..	60	131.0	19.0
23	Tot.	6	6	6	..	..	..	..	..	..	..	..	..	..	..	..	2	2	10	34	60	141.7	38.3
24	Red.	2	2	..	..	..	..	..	..	..	..	..	..	..	..	..	1	9	26	40	169.5	10.5	
25	Tot. No.	27	17	12	19	15	20	38	37	64	231	103	64	84	48	31	60	59	39	72	1040	Rows 1-28 show Frequency of Sd Localization at Possible Positions	
26	Grouped	27	10	8	9	9	22	10	25	42	231	63	41	52	27	24	39	33	25	46	1040	Rows 29-34 show Frequency of Expectation of the Sound	
27	Red. No.	16	10	8	9	9	10	31	25	42	130	63	41	52	27	24	39	33	25	46	640	Rows 29-34 show Frequency of Expectation of the Sound	
28	Grouped	16	10	8	9	9	14	31	25	42	130	63	41	52	27	24	39	33	25	46	640	Rows 29-34 show Frequency of Expectation of the Sound	
29	Unlimited	10	1	..	2	1	..	2	3	2	28	3	6	10	7	12	17	17	9	35	165	Rows 29-34 show Frequency of Expectation of the Sound	
30	Expectations	10	10	..	..	11	..	..	..	..	28	3	6	10	7	12	17	17	9	35	165	Rows 29-34 show Frequency of Expectation of the Sound	
31	Limited	65	16	19	42	22	18	29	14	22	138	17	20	47	18	21	31	39	21	56	655	Rows 29-34 show Frequency of Expectation of the Sound	
32	Expectations	65	16	19	42	22	18	29	14	22	138	17	20	47	18	21	31	39	21	56	655	Rows 29-34 show Frequency of Expectation of the Sound	

In general these results agree, as far as agreement may be expected under our peculiar conditions, with those of previous investigators, with the one slight exception that our data seem to emphasize the tendency of sound-displacement toward the aural axis more than any other observer has so far found.

The next question, then, is to find out whether and to what extent this general tendency is modified by the determination to expect the sound from certain regions of the circumference. For this purpose we may best begin the fractionation of our data with the cases under the instruction "unlimited," because here we have a contrast between observations in which this instruction has been successfully carried out and cases in which the observers of their own accord, whether on account of misunderstanding the instruction or for other reasons, voluntarily limited their expectant attention either to definite points or to certain narrow regions of the scale. The frequency and direction of the voluntary and definitely limited expectations, regardless of individual observers, may be seen in rows 29 and 30 of Table II, while in Table III the frequency distribution of these cases is given with regard to the observers, but without reference to the direction of the voluntarily limited expectation. It will be seen from both Tables that out of the 240 possibilities, 165, or 68.75%, of the cases show the definite voluntary restriction of expectation. This tendency seems to have had no decided influence upon the accuracy of localization, as a comparison with the indefinitely restricted and the entirely unrestricted expectations indicates, according to rubrics b and c in Table I. In 71 of the 165 cases it accidentally happened that the voluntarily selected point of expectation occurred in the same quadrant as the stimulus-position. But in spite of this agreement, the accuracy of localization in these 71 cases was 3.5% less than in the remaining 94 cases. A similar difference is found between rubrics b and c, the voluntary definite restriction giving less accurate results by 3.6% than the entirely and the partly indefinite expectation. If we now compare the combined or "General Results" under "unlimited" instruction, as given in rubric a, with the total results of rubric h, the former seem to be by 3% more accurate than the total. The latter, however, is exaggerated on account of rubric f alone, which presents a special condition to be considered separately later. Without rubric f the total error would be only about 35%, which compared with the errors of rubrics a, b, and c is considerably less. In other words, the instruction "unlimited," whether faithfully carried out or not, interferes decidedly with the accuracy of localization. The introspections offer, as an explanation of this fact, unsteady attention and especially moving visual images. In addition there seems to be a tendency among some of the observers to waver between attempts not to limit expectation and temptations to visualize certain portions of the scale. On the whole the quantitative and introspective data of these experiments under "unlimited" instruction furnish an unlooked-for confirmation of the writer's previous investigation of consciousness under negative instruction.<sup>4</sup>

Considering next the actual direction of expectant attention, when it is thus voluntarily limited to certain points, we find, according to rows 29 and 30 in Table II, that out of 165 cases it was limited in 116 cases to the front half exclusive of  $\pm 90^\circ$ ; if the latter be included,

<sup>4</sup> L. R. Geissler, *Analysis of Consciousness under Negative Instruction*, Am. J. Ps., April, 1912, xxiii, 183-213.

the figure rises to 144. That is to say, only in one out of eight chances do we ordinarily expect a sound to come from behind the ears. A similar tendency is evident in the experiments with limited expectation, where according to instructions all quadrants were equally often represented. Nevertheless the expectations from the front, exclusive of  $\pm 90$ , are by 10% more numerous than those from the back, according to rows 31 and 32 of Table II. The reason for this preponderance of the front half lies probably in the importance of vision for our daily life,—“a sort of racial habit,” as one of our observers called it in his introspections. Among the positions in the front, those of the right quadrant are preferred over those of the left in the ratio 4:3, perhaps because of the righthandedness of our observers. Taken all together, about 50% of the voluntary restrictions of expectation refer to the right front quadrant, about 37½% to the left front quadrant, and only about 12½% to the back half. To summarize, the negative instruction is in a large number of cases understood in the positive sense of “expectation *ad libitum*.” With regard to the influence of expectation, this set of results has been neither definite nor extensive enough to indicate any regularities.

The experiments with positive instruction for definite restrictions of expectation, however, show undisputable evidences of the extent to which expectation may influence the accuracy of sound-localization. As should be expected, our results have not shown a single case in which a stimulus on the right or left has been localized on the opposite side. All confusions or displacements of sound occurred between the front and back halves. The most accurate localizations in this set of experiments seem to have been made when expectation was directed to the right or left half opposite to the actual stimulus-position; cf. rubric d in Table I; while one would have expected the best results when expectation was directed to the same half, that is, under rubrics e and g. This result is, however, somewhat distorted on account of the presence of data with the extreme stimuli 0 and 180 and the absence of data with stimulation  $\pm 90$  in rubrics f and g. Eliminating the results of rows 1 and 13 in rubrics f and g and those of rows 7 and 8 in rubrics d and e, we obtain the corrected results of row 17. But even so rubric d offers the second lowest percentage, while under rubric e the results still show a more marked effect of displacement toward the aural axis than under d or g. The only explanation that we can offer for the difference between d and e,—that is, for the fact that expectation to the opposite right or left half to some extent diminishes the earward displacement and the resulting inaccuracy of sound-localization,—consists in the assumption that such a direction of expectation does away with the kinaesthetic and visual ear-consciousness that we frequently experience in listening to faint sounds and that was explicitly mentioned in the introspections of one of our observers. This explanation is supported by the outcome of the second group of experiments, rubrics i and j, as in these cases the direction of expectation to the quarters did not favor the positions 0, 180, and especially  $\pm 90$ , and therefore was rather applied to the middle of the quarters. Consequently, when the sound was given in the same quadrant to which expectation was directed, the errors were very small, averaging only 29%, while in case the stimulus was in some quadrant different from the one expected, the percentage of error was 36.0; with elimination of the one case of 180 (row 13, rubric i), it is 40.9%. This result is almost the same as that of rubric b, where there was practically no expect-

tation, and where therefore the usual forward and backward confusion observed by practically all our predecessors was influenced least, if at all, by our special instructions. Taking therefore this result of rubric b, about 40%, as our standard for comparison with the influence of definitely directed expectation, we may now proceed to show the quantitative extent of this influence, which itself is of a twofold nature. According to the direction of expectation, its influence upon the forward-backward confusion is either of a restricting or of an enhancing nature.

The former, the restricting influence, is plainly shown under rubric g, where expectation is limited to the same front or back half in which the stimulus is actually given; because in these cases the smallest errors in the displacement of sound occur, averaging a total of 32.7% for all stimuli and a reduced total of 23.8% for the intermediate stimuli exclusive of 0,  $\pm 90$ , and 180. In other words, when expectation is directed to the same front or back half in which the stimulus is to be given, the usual error of sound-displacement due to forward-backward confusion is reduced more than one third. The contrary results occur under rubric f, where expectation is directed to the front or back half opposite to that in which the stimulus is actually given. Under these conditions, the forward-backward confusion is enhanced; the standard error of 40% is increased to 70% if all positions are included, and to 57.6% if the corner-positions are excluded. Considering only the latter figure, we may say that expectation increases the usual error by almost one half. Comparing the absolute differences between the standard error of displacement, 40%, and the modifications due to expectation, we find that the enhancement is practically as great in the one case as the restriction in the other case. These results, then, solve the particular problem for which our experiments had been originally planned. We may summarize our conclusion by saying that the original error of sound-displacement due to the usual forward-backward confusion is either increased or decreased by almost one half according as expectation is directed to the opposite or to the same front or back half from which the stimulus is to be given.

In addition to the results and conclusions thus far reached our data furnish a number of other results which we may now discuss. A study of the mean variations in Table I will show consistent differences with regard to certain stimulus-positions. The mean variations for 0 and 180 are greatest, then follow  $\pm 30$ , and last of all  $\pm 120$  and  $\pm 90$ . This result indicates clearly that the mean variations do not depend upon the inaccuracies of the individual observers, but rather upon the difficulties of localizing sound from certain directions. The question of individual differences must therefore be approached from different angles.

The attitude of the individual observers towards the various kinds of instructions, and their individual degrees of accuracy in localizing sound, are indicated in Table III. In the case of the instruction "unlimited," the last three of the eight women observers (rows 6-8) and the first nine (rows 10-18) of the twelve men are very consistent in voluntarily limiting their expectation to definite points in the circle, as the first three rubrics show; while the other observers seem to waver between this method and no (or at most an indefinite) expectation. With limited expectation the grouping is practically the same, except that one of the women (row 6) joins the wavering group and one of the men (row 19) joins the consistent group. This

difference in attitude seems to have, however, no appreciable influence upon the individual's accuracy of localization, according to the size of the average errors listed under the last four rubrics. Similarly, if we compare all observations under limited indefinite expectation with those under limited definite expectation without regard to the individuals, as is done in Table IV, rubrics k and l, we find, for the first group of experiments, at least, a very small and irregular difference in favor of definitely limited expectation. The same fact holds true with the second group of experiments, except that here the difference is somewhat larger, because most of the cases involved are from observers whose total average errors for the quadrants are considerably larger than the average for all 20 observers. It seems therefore indifferent, for the accuracy of localization, whether one expects the sound from a well-defined point or from a wider and less defined region of the periphery.

TABLE III

Rows	Instruc- tion:		Unlimited or None			Limited Exp'n.		Total Average Errors			
	Expecta- tion:		None	Indef.	Vol. Lim.	Indef.	Defin.	Unlim and H.	Quad- rants	Com- bined	%
	Observers:		1st Group Expts			Both Groups		Both Groups of Expts.			
1	Women	R.R.	6	3	3	24	32	27.7	29.9	28.1	30.6
2		M.E.J.	9	0	3	29	27	32.3	14.4	18.2	30.8
3		P.O.	0	4	8	2	54	43.9	21.2	38.5	42.0
4		S.L.	8	0	4	17	39	43.3	37.5	41.9	45.7
5		H.M.	7	0	5	19	37	45.8	44.4	45.4	49.5
6		J.O.	0	1	11	10	46	25.8	30.0	26.6	29.1
7		J.N.	1	0	11	0	56	30.6	27.5	30.0	32.5
8		D.S.	1	1	10	2	54	45.2	39.4	43.9	47.7
9	Wom. Tot.		32	9	55	103	345	36.8	31.8	35.4	38.5
10	Men	H.M.P.	1	2	9	3	53	49.0	11.2	40.1	43.7
11		H.B.	0	0	12	0	56	43.3	33.1	40.9	44.9
12		L.M.	0	0	12	0	56	42.3	41.2	42.0	45.8
13		H.D.W.	0	0	12	0	56	44.2	39.4	43.1	47.0
14		T.N.H.	0	0	12	1	55	45.8	37.0	43.7	47.6
15		W.E.H.	0	0	12	0	56	46.1	36.2	43.8	47.7
16		E.R.P.	0	0	12	0	56	48.3	33.1	44.7	48.7
17		J.E.M.	0	0	12	1	55	47.3	42.0	46.0	50.2
18		J.M.	1	0	11	0	56	55.5	28.1	49.1	53.5
19		T.A.M.	7	0	5	0	56	42.5	26.2	40.7	44.4
20		G.B.	2	9	1	35	21	45.0	47.5	46.0	50.0
21		F.H.	11	1	0	56	0	41.5	37.5	40.6	44.2
22	Men Total		22	12	110	96	576	46.1	34.4	43.4	47.4
23	Grand Tot.		54	21	165	199	921	42.4	33.3	40.2	43.8

The question of sex-difference in our experiments is complicated by the presence or absence of general scientific and specifically psychological training in our observers. Tables III and IV show a remarkable and consistent difference in accuracy of sound-localization of about 10% in favor of the psychologically untrained eight women observers. This difference cannot be explained on the basis of expectant attitude, because the five women exhibiting the indefinite wavering attitude average 39.7% as against the 45.7% of the three

TABLE IV

Rows	Rubrics	k		l		a		d		e		f		g		i		j		m		n
	Instr.	Total Average and Errors				Unlim. Exp'n.		Exp. Lim. to Right or Left		Exp. Lim. to Front or Back		Exp. Lim. to Quadrant		Total Average and Errors		8 Women		12 Men				
		Indefinite Exp'n.		Definite Exp'n.		General Results		St. opposite		St. same		St. opposite		St. same		St. in diff.		St. in same				
		Rubrics a and d-g				W	M	W	M	W	M	W	M	W	M	W	M	W	M			
		No.	Av.	E.	No.	Av.	E.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	E.	E.	
1	0	7	124	124	53	143	143	122	144	109	163	79	131	109	163	79	131	103.3	103.3	146.0	146.0	
2	+30	26	102	72	114	107	77	116	122	116	132	71	79	114	108	81	94	99.0	69.0	111.4	81.6	
3	-30	23	119	89	117	99	69	100	127	116	132	50	90	111	136	75	92	85.0	55.0	116.5	86.5	
4	+60	15	89	29	125	86	26	94	83	91	112	69	125	95	132	50	90	90.6	30.6	83.7	23.7	
5	-60	20	70	10	120	89	29	109	80	92	79	90	94	104	97	77	74	83.8	23.8	85.0	25.0	
6	+90	14	90	0	46	83	7	92	82	90	92	79	85	...	...	...	...	85.5	4.5	85.3	4.7	
7	-90	5	100	10	55	75	15	97	86	90	87	82	93	...	...	...	...	90.0	0	89.0	1.0	
8	+120	24	95	25	116	96	24	95	89	95	95	99	93	80	84	105	103	97.7	22.3	95.0	25.0	
9	-120	21	104	16	119	97	23	112	103	95	102	89	93	82	90	104	96	96.7	23.3	98.1	21.9	
10	+150	19	126	24	121	131	19	139	129	157	132	129	103	127	103	127	125	135.4	14.6	122.0	28.0	
11	-150	25	132	18	115	127	23	141	138	147	138	124	126	97	102	129	147	130.6	19.4	132.4	17.6	
12	180	12	103	17	48	151	29	132	164	...	...	...	...	94	114	177	161	134.6	45.4	146.3	33.7	
13		211	Expts.	1149	Expts.													416	Expts.	624	Expts.	
Σ Errors		494	484	484	44.8%	43.3%	24.8%	32.5%	65.0%	28.2%	39.4%	40.0%	24.7%	41.8%	30.2%	38.5%	46.0%					
Σ St. Pos.		1080	1080	1080	44.8%	45.4%	35.5%	42.0%	73.6%	39.4%	73.6%	41.8%	30.2%	41.8%	30.2%	38.5%	46.0%					

men showing the same attitude. But the greater frequency of this wavering among the women indicates that, as a whole, they made more frequent attempts to comply with the instruction "unlimited" than the men, of whom only two carried out this instruction in its intended meaning, while the other ten nearly always expected the sound from definite points on the circle. There was also a much greater variety as to training and age among the women than among the men, and therefore these factors cannot be used as a satisfactory basis for explaining the sex-differences. The smallest difference (about 2%) occurred when expectation was to be "unlimited" and when it was directed to quadrants different from those of the stimulus-position (rubrics a and i); next in size is rubric j (5.5%), then g (8.6%), and finally d, e, and f, which vary from 9.5 to 11.2% in their difference; but always the women's results are better than those of the men. With regard to the stimulus-positions no consistent errors between the two sexes can be found, as rubrics m and n will show. We have no suggestions to offer in explanation of these sex-differences.

Our results reveal furthermore to what extent sound-localization is more accurate in various regions of the circumference. Referring from that point of view to Table I again, especially rows 6 and 14, we see that in general (rubric h) the earward displacement from stimulus-positions in the back half (always omitting  $\pm 90$ ) is on an average over twice as great as that from the front half. In particular, this difference is least marked under rubrics j and f, then follow in ascending order rubrics e, g, d, a, and lastly i, where it is greatest. The localization on the right side is uniformly somewhat less accurate than on the left side, the total average errors (exclusive of positions 0 and 180) being 34.7% for right against 31.2% for left; and for the various rubrics the following corresponding pairs of figures obtain, arranged again in ascending order: smallest or no difference for f, 50.4 *vs.* 50.6%; then g, 25.1 *vs.* 23%; d, 28.5 *vs.* 26.5%; a, 34 *vs.* 28.4%; and greatest for e, 37.8 *vs.* 30.5%. This result is somewhat surprising, as practically all our observers were right-handed, a fact which would lead us to expect the opposite result. There is no difference as to sex in this respect, as the corresponding total average errors for the women are right 28.2 *vs.* 25.2% left and for the men 32.6 on the right *vs.* 30.9% on the left side. We have at present no explanation to offer for this superiority of the left over the right ear.

We are now ready to discuss the introspective reports on the mental processes of expecting and localizing sound as required from ten of our male observers. These reports are given here in full, partly for the sake of avoiding arbitrary selection, and partly in order to show the quality of introspective work which may be expected and obtained from undergraduates with average and better than average college records. The observers were told to subdivide their report into a pre-stimulation or expectation-period (A), including the conscious facts from the perception of the instruction up to the perception of the stimulus, and a localization-period (B), beginning with the sound perception. The observers either wrote out their reports themselves or dictated them to the recorder. No suggestive questions were asked by the latter; but whenever a word or phrase in the report did not seem clear to him, he demanded further details, which were to be expressed as far as possible in technical terms. The reports are numbered in accordance with the rows 10 to 21 of Table III. The small letters in parenthesis in the first part of each report refer to the cor-



responding items in the subsequent summary outline of the expectation-period, while the bracketed letters in the second part correspond to the items in a similar outline of the localization-period. The abbreviations employed are either self-evident or will be explained in the subsequent outlines.

10. A. Vis. Im. of instrument from where expected (a). Im. is projected into external space (t). Musc.-Kin. Tendency of eyes to turn (l) toward expected im. Kin. Tend. to turn head (f) toward expected noise. With "Unlimited" instruction (o) the position of instrument located by projected image (t).

B. Auditory sensation (a). If it comes from unexpected place, the original vis. im. fades and a new vis. im. (e) appears. Kin. Tend. to straighten up after sound localisation (j). If uncertain about right place, tend. to turn head (h). Sensation is pl. (q) when coming from expected direction, otherwise slightly unpl. (r). Vis. and auditory im. (e) of telephone receiver.

11. A. Conscious of kin. strains in head (f) and esp. in forehead. Kin. tend. to turn eyes (l) toward expectation-point when it was in the right or left front quadrants (n). When exp'n. was to back half (n), a kin. tendency to turn entire head (f). Whenever expected from 180°, a faint aching pressure (q) in middle of forehead and slightly toward right eye. Exp'n. of direction occurred only as kin. tend. of turning toward certain point in scale, which later in the report is translated into verbal ideas of figures [cf. B. (o)—]. All exp'n figures should be modified by the word "about" (t). In two or three cases a slight jar of apparatus gave a cue for exp'n. (s), although instructions for one or two of these were in a different direction. In the other cases the jar did not influence the direction of expectation (t).

B. After perception of sound (a), kin. tend. to nod head in direction of lines (h). Verbal ideas of reporting (o).

12. A. Vis. Im. of circle and points in circle (b). Sometimes kin. pointing in direction of instruction (k). Perhaps also eye-movements (h) in same direction. Visualised instrument at certain point (a). Several times did not expect sound according to instruction (r).

B. Sound had a space-attribute at once, and afterwards pointing, (b) and (m). Visualised a large circle on floor (g), sometimes with Vis. im. of instrument at certain point (e). Sometimes a feeling of surprise (s) slightly unpl. (r), when sound came from another direction.

13. A. Had a visual im. of the part of scale (b) from which sound was to be expected, divided into degrees. Sometimes also verbal im. of certain parts in that scale (e). Kin. strain located in neck (g) and back (j), as if leaning in required direction. When instruction was "unlimited" (o), had a vis. im. of the four points 0,  $\pm 90$ , and 180 (c), and whichever came first into mind was the one from which sound was usually expected.

B. The auditory perception (a) of the sound aroused a vis. im. of the instrument in the particular position (e). Pointed (m) or tended to point (i) in the direction from which the sound was heard, and then translated this into degrees of the scale, (o).

15. A. At the words "Unlimited" (o) a visual image of instrument either at 180 or at 0 (c). Just before signal "Ready" a vis. im. of instrument in front half turning from left to right (d). At the words "Ready" a vis. im. of instrument at some definite spot (a). Sometimes there is a delay in visualising the apparatus, (t).

B. Auditory sensation (a), pleasant when sound came from expected direction (q). Tendency to laugh (n) and nod head in direction from which sound came (h). Verbal ideas of translating vis. im. into terms of scale (o).

17. A. When starting in, my idea was that I could not tell much about it. Was surprised because sound seemed to come from a definite point, clear cut (t), and has been so every time except in a few cases. I had thought also that the point of expectation had nothing to do with it at all (t). At first I had trouble in locating degrees, but later a pretty good vis. im. of instrument (a), of scale with — and + posts, the zero and 180 (b) and (c). When instructed to expect sound from given half (p), I usually imaged the central point of it. When the right or left half was called for, had a peculiar feeling in one of the ears, very dim, resembling a cutaneous pressure (p), a bare consciousness of one ear (t). Sometimes had a musc. tend. in the neck (m) as if leaning the head in the direction of expectation (n). Sometimes my attention wandered to some other point of the scale (s), but almost always returned to the place told.

B. The sound (a) was heard to come from certain points (b). A kin. tend. to point to it (i), and thus the sound locates itself. The placing on the scale was mostly in verbal terms (o).

18. A. Kin. strains all over body (i). Attention directed to some point located in scale (c). Vis. im. of apparatus (a) moving toward this point (d). Vis. im. of a large circle marked in 10° points (b) and (e).

B. Auditory perception of click of instrument (a). Previous vis. im. disappear and are replaced by verbal kin. perceptions and ideas (o) of giving the position of the click. Sometimes attention fluctuated between vis. im. (u) of points 180° apart. Sometimes additional vis. im. of experimenter and recorder and of windows with strip of wall between (f).

19. A. Vis. im. of dial with pointer (a) and (b) with a special reference to red figures (c). The four points 0,  $\pm 90$ , and 180 were esp. plain (c). Associated app. with a wheel with hub and four spokes at right angles (q). The vis. im. continually went back to 90 (d), but sometimes thinking I would be fooled by experimenter (r), I changed it to other points. If "Unlimited" (o), this figure seemed to revolve without stopping (d) at any place.

B. Localization was accomplished (a) either by actually pointing (m) or imaging myself pointing (k) in the direction of sound. Had a vis. im. of scale at all times (c). Imagined a straight line from sound to myself and up to myself, horizontal lines seemed about ten feet long, (g), as sound seemed to come from far distance and somewhat from below.

20. A. Tried to discover, against my will (t), what the experimenter's regular order of stimuli was; in verbal ideas (e). Expected the stim. in kin. imagery, felt that I could move or point (k) toward

the place without hearing the sound. Had a vis. im. of red letters on scale (c) and verbal idea (e) that the sound would come more frequently from their direction (t). Vis. im. of the two parts of the app. moving (d) set up by the Experimenter's steps (s) and a verbal idea that sound must come from either the one or the other point (r). My kin. position of whole body and chair never felt satisfactory in that it seemed not in the center of the circle (i) and (t). This may be one source of my errors. Had a stronger tend. to locate points in front rather than behind (t), because everything seems to be projected forward in everyday life, a sort of racial habit. Several times I thought that Experimenter would fool me and expected from another than the instructed direction (r).

B. Localization through auditory perception (b) associated with vis. memory im. of instrument and scale (c) and (e). The localization aided by expectation because I concentrated steadily in the expected region (t).

21. A. Imagined myself in center of circle divided up in degrees in vis. terms (b). When Experimenter said which half, I direct my att'n visually to that half as a whole (p), unless some noise by E caused me to pick out special place in vis. im. (s). In these cases I was inclined to lean my head in its direction (m). In second group of experiments I would have a vis. im. of map of quadrant and tend to lean my ear toward it (n) and forget about rest of circle (t). Sometimes a vis. im. moving as of app. moving (d). Twice I expected sound in opposite field from that told, because I had a verbal idea (e) that E. was trying to deceive me (r), and I reasoned out that he would put it somewhere else.

B. With aud. perc. of noise (a) I tried to select on vis. map (c) the nearest place from which I thought the sound came (d), and figure out in verbal ideas (o) the location by adding and subtracting. Sometimes I located sound first by pointing (m), which was sometimes easier, but not always. Had a tend. to bend head (h) in direction, and in some cases, esp. in group 2, I actually bent it toward sound (l). Attention was always concentrated on work (t) and affectively it was mainly indifferent (p).

The introspective items of the expectation-period may be summarized as follows in order to represent the conscious pattern of expectation. The numbers behind each item refer to the different reports in which the particular item has occurred.

#### A. *Expectation-Period*

##### Visual Imagery

- (a) of instrument (sometimes in certain position) 10, 13, 15, 17, 18;
- (b) of whole scale 13, 14, 17, 18, 19, 21;
- (c) of special parts of scale 14, 15, 17, 19, 20;
- (d) of moving instrument 15, 18, 19, 20, 21;

##### Verbal Imagery and Associations (e) 14, 18, 20, 21;

##### Kinaesthetic Sensations of Strain and Actual Movements

- (f) located in head (10), 11;
- (g) located in neck 14;
- (h) located in eyes 13;
- (i) in general or various parts of body 14, 18, 20;

## Kinaesthetic Imagery and Tendencies

- (k) of pointing 13, 20;
- (l) of turning or moving eyes 10, (11);
- (m) of moving head (11) 17, 21;
- (n) of movements in special directions 10, 11, 17, 21;
- Special Sensations, Imagery, and Associations
  - (o) for "unlimited" instruction 10, 14, 15, 19;
  - (p) for Halves or Quadrants 17, 21;
  - (q) in general 19, for special points 11;
- Expectation voluntarily changed from instruction
  - (r) by auto-suggestion 13, 17, 19, 20, 21;
  - (s) by accidental noises, etc., 11, 20, 21;
- Miscellaneous
  - (t) 10, 11, 14, 15, 17, 20, 21.

A corresponding outline may be made for

B. *Localization-Period*

## Perception of Sound

- (a) unqualified, by all observers
- (b) with direct spatial attribute 13, 17, 20;

## Visual Imagery

- (c) of scale as a whole 11, 19, 20, 21;
- (d) of points on scale 21;
- (e) of whole apparatus in certain position 10, 13, 14, 20;
- (f) of environment 18;
- (g) of imaginary aids 13, 19;

## Kinaesthetic Processes

- (h) tendency to move or turn head 10, 11, 15, 21;
- (i) tendency to point finger 14, 17;
- (j) tendency to move whole body 10;
- (n) tendency to laugh 15;
- (k) images of pointing 19;
- (l) actual movements of head 21;
- (m) actual movements of fingers 13, 14, 19, 21;

## Verbal Ideas of Translating localizations and of Reports

- (o) 11, 14, 15, 17, 18, 21;

## Affective Processes

- (p) indifference 21;
- (q) pleasantness 10, 15;
- (r) unpleasantness 10, 13;
- (s) surprise 13;

## Attentive States

- (t) steady concentration 20, 21;
- (u) fluctuations 18.

These introspective data are not only in full accord with the numerical data from the same observers, but they also cast some light on the processes of expectation and localization of sound and on the tendency of confusing front and back. With regard to the first they show, on the one hand, that the positions 0,  $\pm 90$ , and 180 are especially emphasized by the visualizing expectation through greater frequency and vividness than any other positions; and, on the other hand, that the prevalence of the positions in the front half seems to be due to the predominating influence of vision upon our whole mental life. With regard to the localization of sound, the reports show the impor-

tance of the kinaesthetic and cutaneous processes originating about the head, neck, and eyes. They finally suggest as an explanation of the frequent earward displacement of sound the usual bare ear-consciousness resembling a dim, cutaneous pressure. Nothing is said in these reports about a consciousness of the instruction, because, as must be remembered, the auditory perception of the instruction usually aroused at once and more or less mechanically a definite visual image, sometimes supplemented by kinaesthetic processes, of the region from which the sound was to be expected. With perhaps one exception none of the observers seems to have suspected the least influence of the direction of expectation upon his accuracy of localization, and as for the degree of accuracy itself, all observers seemed to be absolutely at sea at the end of the session. It is, finally, interesting to note that only three observers attributed more or less explicitly a spatial characteristic to sound, while the others make sound-localization an indirect function depending upon the spatial characteristics of the secondary criteria, such as the associated visual and kinaesthetic processes. But we do not wish to emphasize these and other minor details, because it was not our intention, in these brief introspective reports, to secure an analysis of the conscious mechanism of sound-localization.

We may conclude this study with the following summary of our main results and conclusions:

(1) When sound-stimuli are presented in a horizontal plane at the level of the ears, their localization shows a definite confusion of front and back with a tendency to mass the localizations around the aural axis.

(2) This tendency is strongest with sounds coming from the back.

(3) It is either enhanced or diminished to the same extent,—that is, by almost half of the original error due to front-back confusion,—by the proper direction of expectation to or away from the regions of the stimulus-positions.

(4) The tendency toward earward displacement is perhaps due to a general kinaesthetic and cutaneous "ear-consciousness."

(5) Under the negative instruction "unlimited" or "no expectation" the accuracy of sound-localization decreases by 10% of the general average error.

(6) This negative instruction is as a rule interpreted in the positive sense of "expectation *ad libitum*."

(7) Under this instruction sounds are expected in 87½% of the cases from the front half and only in 12½% from the back. In general, sounds are also more often localized in front than in the back.

(8) This predominance of the front half over the back half in expectation and localization of sound seems to be due to the great influence of vision upon the whole of mental life.

(9) The women observers without previous psychological knowledge were about 10% more accurate in localizing sound than the men who had a uniform and moderate amount of preliminary psychological training.

(10) The left ear was found to be uniformly more accurate by about 3% than the right ear.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XXVII. THE INFLUENCE OF SUPPRESSING ARTICULATION ON THE FAVORABLE EFFECT OF DISTRIBUTING REPETITIONS

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By MILDRED MOULD, LOIS TREADWELL, and M. F. WASHBURN

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In Study Number XXIII from this laboratory, which appeared in the AMERICAN JOURNAL OF PSYCHOLOGY, vol. 24, pp. 580-582, it was shown that a series of hand and arm movements could be learned with fewer repetitions if an interval of one minute was allowed between the repetitions than if the series of movements was performed again and again without interval. Attention was of course distracted to prevent conscious recall of the movements during the one minute interval. This instance of the working of Jost's Law in a learning process where the motor factors involved were not articulatory suggested the possibility that the law might be primarily concerned with motor processes. In the present study we undertook to investigate the value of the one-minute interval in the learning of nonsense syllables where the articulatory or motor factors were strengthened and where they were interfered with or weakened. If Jost's Law is primarily a law of the learning of movements, then we should expect to find the one-minute interval most favorable when the motor processes in learning syllables are most strongly emphasized.

To find out whether this was the case, we adopted a method which enabled us to work with a number of observers almost unprecedented in memory investigations, and as in the case of Study Number XXIII allowed us wholly to eliminate the effects of practice. It was based on the Method of Retained Members. Six series of ten nonsense syllables each were prepared according to the rules laid down by Mueller and Schumann. They were called Series A, B, C, D, E, and F. These series were used by each observer in the same order, that is, A was always the first one used, then series B, and so on to series F. The syllables were printed each on a separate card, and were laid singly and successively before the observer in time to the beats of a metronome set at sixty; two beats were allowed for each exposure and two for the change of cards. The series was in every case presented four times to the observer, who at the end of the fourth presentation recalled as many of the syllables as she could. In the case of two of the six series presented to a given observer, she was instructed to pronounce each syllable silently as she looked at it. In the case of two other series, she was instructed to try to avoid pronouncing the syllables to herself, and to learn them as nearly as possible wholly by vision. In the case of the remaining two she was instructed to articulate silently, as she looked at each syllable, the syllable 'Deb.' These three methods will be referred to as the articulation method, the no-articulation method, and the Deb method. With one of the two series apprehended by each method, an interval of one minute, during which the observer's

attention was distracted, was allowed to elapse between each presentation of a series and the next. With the other one, the interval between presentations was only four seconds, just enough to mark the end of a presentation.

The influence of practice and of the varying difficulty of the series was eliminated in the following way. Twelve different orders of experiment were used, each with a different observer. The first order was: Series A, by the no articulation method, with four second interval; Series B, no articulation, one-minute interval; Series C, articulation method, four seconds interval; Series D, articulation method, one-minute interval; Series E, Deb method, four seconds interval; Series F, Deb method, one-minute interval. The second order was: Series A, no articulation, one-minute interval; Series B, no articulation, four seconds interval; Series C, articulation, one-minute interval; Series D, articulation, four seconds interval; Series E, Deb method, one minute interval; Series F, Deb method, four seconds interval. The third order was: Series A, no articulation, four seconds interval; Series B, no articulation, one-minute interval; Series C, Deb method, four seconds interval; Series D, Deb method, one-minute interval; Series E, articulation, four seconds interval; Series F, articulation, one-minute interval. The fourth order was: Series A, no articulation, one-minute interval; Series B, no articulation, four seconds interval; Series C, Deb method, one-minute interval; Series D, Deb method, four seconds interval; Series E, articulation, one minute interval; Series F, articulation, four seconds interval. The fifth order was: Series A, articulation, four seconds interval; Series B, articulation, one-minute interval; Series C, no articulation, four seconds interval; Series D, no articulation, one-minute interval; Series E, Deb method, four seconds interval; Series F, Deb method, one minute interval.\* The sixth order was: Series A, articulation, one-minute interval; Series B, articulation, four seconds interval; Series C, no articulation, one-minute interval; Series D, no articulation, four seconds interval; Series E, Deb method, one-minute interval; Series F, Deb method, four seconds interval. The seventh order was: Series A, articulation, four seconds interval; Series B, articulation, one-minute interval; Series C, Deb method, four seconds interval; Series D, Deb method, one-minute interval; Series E, no articulation, four seconds interval; Series F, no articulation, one-minute interval. The eighth order was: Series A, articulation, one-minute interval; Series B, articulation, four seconds interval; Series C, Deb method, one-minute interval; Series D, Deb method, four seconds interval; Series E, no articulation, one minute interval; Series F, no articulation, four seconds interval. The ninth order was: Series A, Deb method, four seconds interval; Series B, Deb method, one-minute interval; Series C, articulation, four seconds interval; Series D, articulation, one-minute interval; Series E, no articulation, one-minute interval; Series F, no articulation, four seconds interval. The tenth order was: Series A, Deb method, one-minute interval; Series B, Deb method, four seconds interval; Series C, articulation, one-minute interval; Series D, articulation, four seconds interval; Series E, no articulation, one-minute interval; Series F, no articulation, four seconds interval. The eleventh order was: Series A, Deb method, four seconds interval; Series B, Deb method, one-minute interval; Series C, no articulation, four seconds interval; Series D, no articulation, one-minute interval; Series E, articulation, four seconds interval; Series F, articulation, one minute interval.

The twelfth order was: Series A, Deb method, one-minute interval; Series B, Deb method, four seconds interval; Series C, no articulation, one-minute interval; Series D, no articulation, four seconds interval; Series E, articulation, one-minute interval; Series F, articulation, four seconds interval.

Thus each of the six methods of apprehension of the syllables was used with each series and in each time position. Results were obtained from twelve sets of twelve observers each, and each observer, of course, had performed a complete experiment involving the presentation of six series. There were thus one hundred and forty-four observers in all; there was no practice error, and no error due to unequal difficulty of the series.

The results were in the form of the number of syllables correctly retained after four presentations of a series. If a syllable reproduced had two letters in common with one that belonged to the series, but the third letter wrong, it was counted as two-thirds right. The total number of syllables retained by the hundred and forty-four observers when each syllable was silently articulated was, with the four seconds interval, 1,001; with the one-minute interval, it was 1,075.4. The one-minute interval gave an advantage of 74.4 syllables, or 7.3 per cent. of the number learned with the four seconds interval. When the syllables were learned with the attempt to suppress articulation, the total number learned was with the four seconds interval 847.9; with the one-minute interval 879.9, showing a gain for the one-minute interval of 32 syllables, or 3.7 per cent. of the number learned with the four seconds interval. When the syllables were accompanied by the silent articulation of the syllable Deb, the total number learned was, with the four seconds interval, 902.3; with the one-minute interval, 920.0. The amount gained with the one minute interval was thus 17.8 syllables, or 1.9 per cent. of the amount learned with the four seconds interval.

The conclusion that unmistakably follows from these experiments is thus that the advantage which an interval of one minute between repetitions gives over an interval of four seconds only is decidedly greater, about twice as great, when the motor processes accompanying learning are emphasized than when the attempt is made voluntarily to suppress them; and more than three times as great with emphasis on the motor processes than with the suppression of them by the substitution of other motor processes. That such voluntary or artificial suppression of the motor processes could not have been complete; that some articulation of the syllables must have successfully resisted it and even accompanied the articulation of 'Deb' is obvious, but there can be little doubt that the association of the motor processes involved in reading the syllables was more effectually secured when they were given free play than when they were interfered with. It will be seen from the figures given above that both the effort to suppress articulation and the silent pronouncing of 'Deb' operated as distractions; the total number of syllables recalled with full articulation was 2,076.4; with effort to suppress articulation it was 1,727.8; with articulation of 'Deb' it was 1,822.3. It also appears that 'Deb' proved less of a distraction than the effort to suppress articulation; perhaps the silent pronouncing of 'Deb' as each syllable was looked at served to steady attention and keep it from wandering.

Within our experimental conditions, we have good evidence that the law of the superiority of distributed repetitions is chiefly concerned with the motor aspect of learning.



## XXVIII. AFFECTIVE SENSITIVENESS TO COLORS, TONE INTERVALS, AND ARTICULATE SOUNDS

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By MARGARET BABBITT, MARJORY WOODS, and M. F. WASHBURN

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In Number XXIII of the Minor Studies from this laboratory (this JOURNAL, volume 24, pages 583-585), it was suggested that a numerical value might be obtained which should represent an individual trait hitherto not investigated as such, namely, the degree of affective sensitiveness to certain classes of impressions. By affective sensitiveness was meant the tendency to derive extreme degrees of pleasantness or unpleasantness from a class of stimuli. Evidently affective sensitiveness may be either general, in other words a tendency to have strong affective reactions to all kinds of stimuli, or specialized, that is, a tendency to be strongly stirred affectively by one kind of impression but not by others. The numerical measure which was suggested for affective sensitiveness was as follows: The observer was required to express the pleasantness or unpleasantness of a number of stimuli belonging to a certain class by the use of the numbers 1 to 7; the ratio was then found between the number of '4' or indifference judgments to the number of '1' or very unpleasant plus the number of '7' or very pleasant judgments. This ratio evidently varied inversely with the degree of affective sensitiveness, and is trustworthy so long as neither the numerator nor the denominator is zero.

In the study above referred to, affective sensitiveness to colors and articulate sounds was investigated. In the experiments described in the present study we used three kinds of material; small pieces of the Bradley colored papers (2.9 cm. square) on a white ground; nonsense syllables consisting of an initial vowel and a final consonant; and musical intervals and chords struck on the piano. The object in using three kinds of stimuli was to study further the relations between general and special affective sensibility; to see, for example, whether the existence of special affective sensibility to auditory impressions would be indicated by a higher correlation between sensibility to tones and sensibility to articulate sounds than between either and colors. There were ninety colors (the full Bradley set), ninety syllables, and thirty intervals and chords. The latter comprised all the intervals within the octave, and a number of three toned chords, consonant and dissonant. Since if the musical stimuli had been given one immediately after the other the sequence would have affected their pleasantness, the order of presentation was as follows. First, three colors were laid one after the other on a white ground, and the observer was asked to judge their pleasantness or unpleasantness using the numbers 1 to 7. Then one of the intervals or chords was struck on the piano, and the observer recorded her judgment of its affective value in the same way. Then three syllables were pronounced, with equal force and distinctness, and the observer's judgment of the pleasantness or unpleasantness of each was recorded. The proceeding was continued until judgments had been made of all the colors, tones, and syllables. The same order was used for the individual colors, tones, and syllables with each observer.

There were ninety-seven observers, and the index of affective sensitiveness was calculated for colors, tones, and syllables in the case of each. Since some of the indexes had no finite value owing to the

fact that the observers either made no indifference judgments or no judgments of extreme pleasantness or unpleasantness, so that the numerator or denominator of the ratio was zero, it was not possible to get averages. The observers were arranged in order of affective sensitiveness to each kind of impression, those who made no indifference judgments standing at the head of the list in the order of the number of extreme judgments they made, as the most sensitive; and those who made no extreme judgments standing at the bottom in the inverse order of the number of indifference judgments they made, as the least sensitive. This arrangement is rather rough, but may serve where the number of observers is so large. Pearson rank-difference coefficients of correlation could then be found.

The following results appeared. First, the highest correlation, 36 per cent., existed between affective sensibility to colors and affective sensibility to syllables; that is, where tones were left out. The correlation between syllables and tones was 31 per cent., and that between tones and colors was the lowest, 24 per cent. It thus looks as if there exists a specialized affective sensitiveness to tones, which is responsible for a lower degree of correlation between sensitiveness to tones and that to other kinds of impression. Considering the special character of musical ability this is what we should expect. It was not possible to show, however, that the observers who were particularly sensitive to the affective values of musical intervals and chords were the most musically gifted, nor that those who were specially insensitive had 'no ear,' so far as their own testimony went; no tests were made. The matter was complicated by the fact that the musical observers had often studied harmony, and a chord which the naïve observer might have pronounced very unpleasant was to them 'interesting' by virtue of its significance for resolution. It would also appear from our indexes that there exists a slight degree of special affective sensitiveness to auditory impressions as such, which is responsible for the higher correlation between tones and syllables as compared with that between tones and colors.

Secondly, our results show that affective sensitiveness to tones is greater than that to colors, but only slightly, and that affective sensitiveness to articulate sounds is markedly less than that to either tones or colors. While it was not possible to average the indexes of affective sensitiveness, this conclusion may be based on the following figures. The number of indexes below 1 in value, indicating high and fairly high affective sensitiveness, was for tones 64, for colors 60, and for syllables only 29. The number of observers who never made a judgment of indifference was for tones, 10, for syllables 1, for colors, 1. The number of observers who never made a judgment of either extreme pleasantness or extreme unpleasantness was for tones, 0, for colors, 0, and for syllables, 6.

Finally, our results tend to indicate that general indifference is a more marked phenomenon than general affective sensitiveness. The lists of the first thirty observers as regards affective sensitiveness to each of the three classes of stimuli contain nine names that are common to all three lists (33 per cent.), representing those observers whose affective sensitiveness was general, who tended to be strongly affected, pleasantly or unpleasantly, by all the classes of stimuli. The lists of the last thirty observers as regards affective sensitiveness to each of the three kinds of stimuli contain thirteen names which are common to all three lists (43 per cent.), representing those observers who were markedly indifferent to all kinds of stimuli used. If this excess

of general indifference over general sensitiveness should prove to be a universal law, it might well be due to a failure on the part of a certain number of observers to give adequate attention to the stimuli, which would result not only in indifference judgments but in the suppression of whatever special influences might otherwise be exerted by special classes of stimuli.

## XXIX. THE INFLUENCE OF FATIGUE ON AFFECTIVE SENSITIVENESS TO COLORS

By HARRIET ROBBINS, DOROTHY SMITH, and M. F. WASHBURN

In Minor Study Number XIV from the Vassar laboratory (this JOURNAL, volume 22, pp. 112-114), there were reported some experiments tending to show that colors are judged to be less agreeable at the end of a long series of such judgments than at the beginning; that is, that the fatigue induced by a long series of judgments on the affective values of colors lowers their affective value. Our object in the present study was to see how fatigue thus brought about would influence the affective sensitiveness of the observer to the colors; that is, not his tendency to find the colors pleasant or unpleasant, but his tendency to make judgments of an extreme rather than a moderate degree of either pleasantness or unpleasantness. Our method was somewhat more exact than that used in the former study.

The ninety colors of the Bradley series, represented by pieces of the colored papers 2.9 cm. square, were divided into four sets, designated as A, B, C, and D. Sets A and C were of twenty-two colors each; sets B and D of twenty-three colors each. The colors were chosen in random order to make the sets, but the pieces of paper were then numbered on the back so that the colors of a set should always be presented in the same order. Each piece of colored paper was laid by itself on a white background before the observer, who was asked to judge its pleasantness or unpleasantness using the numbers 1 to 7 in the usual way. The entire ninety colors (four sets) were presented thus twice over without pause, making 180 judgments of affective value at a sitting. The set that was used first at a sitting was also used last at the same sitting; thus, one order was: A, B, C, D, B, C, D, A; another, B, C, D, A, C, D, A, B. The observer was asked at the close of a sitting whether she felt tired or bored. For the series which had been presented at the beginning of the sitting and again at the end the following numerical values were calculated; the average affective value of the colors on their first presentation and on their second presentation (that is, the numbers obtained by averaging the numerical values assigned to the individual colors of the set), and secondly, the coefficients of affective sensitiveness for the colors on their first and on their second presentation (that is, the number of judgments '4,' or indifferent, divided by the sum of the numbers of '1,' very unpleasant, judgments and '7,' very pleasant judgments.)

There were 82 observers, all young women and undergraduate students. We were rather surprised to find that only 31 per cent. of them declared themselves bored at the end of the long series of 180 judgments. Five of the 82 observers said that they became more interested towards the end than they had been at the beginning.

Of those observers who reported being conscious of boredom, 34.6 per cent. showed higher affective values for the colors at the end of the series than for the same colors at the beginning, and 65 per cent. showed lower affective values. A difference of 30.4 per cent. represented the tendency of affective values to drop as the result of ennui. Of those observers who reported equal interest maintained throughout the experiment, 36 per cent. gave an average affective value higher for the colors on their second presentation, and 56 per cent. an average affective value lower for the colors on the second presentation: a difference of 20 per cent. indicating the observers who in spite of not recognizing boredom introspectively, showed a fall in the average degree of pleasantness assigned to the colors at the end of the series. Of the five observers who said they were more interested in the colors at the end than at the beginning, three showed a rise in the average affective values assigned to the colors and two a fall.

Turning to the coefficients of affective sensibility, we find that among the observers who reported ennui, 23 per cent. showed increased affective sensibility and 53 per cent. decreased affective sensibility, a difference of 20 per cent. in favor of the conclusion that ennui lowers the affective sensibility. Among the observers who reported equal interest throughout, 43 per cent. showed increased affective sensibility and 38 per cent. decreased affective sensibility. All of the five observers who said their interest increased showed increased affective sensibility.

These results indicate that, under the conditions of our experiments, affective sensibility to colors tends to diminish with ennui produced by a long series of judgments on the affective values of colors, and that diminution in affective sensibility is more closely correlated with introspective reports of ennui than is the average affective value of the colors. The percentages of observers who showed a lowering of the affective values of the colors were 65 for the observers who reported ennui, 56 for those who reported equal interest throughout, and 40 for the few who reported increased interest. The percentages of observers showing decreased affective sensibility were 53 for those reporting ennui, 38 for those reporting equal interest, and 0 for those reporting increased interest.

### XXX. THE SOURCE OF AFFECTIVE REACTIONS TO ARTICULATE SOUNDS

By LOUISE N. GARVER, JOSEPHINE M. GLEASON, and M. F. WASHBURN

So far as we know, the first experimental investigation of the pleasantness and unpleasantness produced by articulate sounds was made in this laboratory, the results being published in this JOURNAL, volume 23, pp. 579-583. The object of this study was to find what consonants and what vowels are most agreeable and most disagreeable when used in nonsense syllables composed of an initial vowel and a final consonant. During the experiments made in the investigation the question naturally suggested itself as to why certain articulate sounds should be agreeable and others disagreeable. The present study is an attempt to answer this question.

The material was again nonsense syllables composed of an initial vowel and a final consonant. The vowel sounds used were a as in ate, a as in father, a as in hat, aw, ee, e as in wet, i as in life, i

as in hit, o as in ope, o as in hot, oo, u as in hut, ow, and oi. The consonants included the hard checks p, t, and k; the soft checks b, d, and g; the breaths f, v, th as in breath, th as in breathe, sh, zh, s, and z. Each syllable was pronounced clearly to the observer, who was asked to record her judgment of its pleasantness or unpleasantness by using the numbers 1 to 7, and after doing so to give from introspection the reasons which made the sound seem pleasant or unpleasant. The total number of syllables used with each observer was 151, all combinations making sense being excluded. The syllables were presented in the same order with each observer, so as to exclude the factor of affective contrast. The total number of observers was sixty-five. The great majority were, as usually in our studies, young women college students, but there were seven persons of greater maturity among the number, and also seven men, too small a number, of course, to permit of any comparison between groups.

Two sources at once suggest themselves for the affective reactions made to meaningless articulate sounds: the associative suggestions which the sounds have in spite of their meaningless character, and the ease or difficulty of the articulatory movements involved in pronouncing them. The first conclusion which we have been able to draw from a study of our results is that extreme judgments of pleasantness or unpleasantness, judgments of 'very pleasant' or 'very unpleasant,' are more apt to be due to associations than are judgments of moderate pleasantness or unpleasantness. The per cent. of 'very unpleasant' judgments due to associations with the nonsense syllables was 24; the per cent. of 'moderately unpleasant' judgments due to the same cause was 12.3; that of 'slightly unpleasant' judgments was 12.6; that of 'slightly pleasant' judgments was 13.4; that of 'moderately pleasant' judgments 16.4; that of 'very pleasant' judgments 29.9. We might conclude that there is little about the character of an articulate sound itself, aside from the associations it suggests, to give rise to extreme affective reactions.

The nature of the associations suggested by the sounds may be briefly summarized. First, there were many associations made directly with words, as when the sound 'ool' suggested 'pool.' Or the word association was indirectly made, as when 'ög' suggested 'fairy tale,' obviously through 'ogre.' Secondly, a sound often suggested the object making a similar sound: 'ees' suggested mice. Thirdly, a sound suggested that it was a corruption of a word through dialect or some defect of articulation. Fourthly, a sound sometimes suggested a sensation associated with a word meaning: 'ife' felt sharp, 'oother' felt like a hand stroking the shoulders; both clearly indirect word associations. Finally, there were a number of unaccountable associations with the sounds. 'Oig' suggested a sickly grin, 'ofe' a witch, 'oif' a giant, and so on. Very likely these were cases of 'mediate' verbal associations.

It appears, as a second conclusion, from the figures given above that associations contribute more to the pleasantness than to the unpleasantness of articulate sounds. The aggregate of the percentages of unpleasant judgments due to association was 48.9; that of the percentages of pleasant judgments due to association was 59.7.

A third conclusion is that the pleasantness or unpleasantness derived from ease or difficulty of articulation is more likely to be of a moderate than of an extreme character. 2.8 per cent. of the judgments 'very unpleasant' were due to the unpleasantness and difficulty of articulation; 3.7 per cent. of the 'moderately unpleasant' judgments,

5.5 per cent. of the 'slightly unpleasant' judgments. 4.2 per cent. of the 'slightly pleasant' judgments were assigned to ease and agreeableness of articulatory movements as a cause; 4 per cent. of the 'moderately pleasant' judgments, and 3.2 per cent. of the 'very pleasant' judgments. It will be noted, finally, that the balance here is a little on the side of unpleasantness; the percentage of judgments 'slightly unpleasant' is the largest due to articulatory influences.

The cases where no reason was given for finding a sound pleasant or unpleasant include two classes which we have not attempted to separate: those where the unpleasantness or pleasantness may really have belonged to the sound itself, and those where the observer's introspection was unequal to the task of detecting the reasons for her own reaction. It is a question which goes to the root of aesthetics, whether a sensation can have any affective character that does not trace its cause to welfare. Our likes and dislikes are traditionally explained as due to conditions that favor or hinder our own good, including that of our species. How can finding a color pleasant or unpleasant have survival value? We can connect articulate sounds with welfare through their associations and through the benefit of free movement of the articulatory mechanisms and the harm of impeded movements. The impossibility of distinguishing between the effects of imperfect introspection and those of affective reactions to pure sounds, aside from associative or kinaesthetic elements, makes us unable to draw any conclusions with regard to the occurrence of such reactions.

Certain observers reacted separately to the consonant and to the vowel in a syllable; that is, they declared that they liked or disliked both, or liked one and disliked the other. In this case the vowel sounds were nearly always declared to be simply agreeable or disagreeable, without further qualification. The consonants, on the other hand, often had some more definitely qualifying adjective applied to them. Occasionally these adjectives referred to an association suggested by the consonant itself, as 'lispings' for *th*. In other instances the adjectives were descriptive of the consonant sound itself. The checks, especially *p*, *b*, and *d*, were objected to as cutting short the vowel sound too abruptly, as being sudden. On the other hand *l*, *m*, and *th* as in *breathe* were especially commended for the opposite character of being 'continuous,' 'letting the sound out.' Does the dislike of an abrupt close to the vowel sound rest upon the general association of abrupt movements with violence and constraint? The term 'hard' or 'harsh' was commonly applied to the guttural checks, which are the most unpopular consonants. It is difficult to decide whether this refers more to the abrupt character of these modifications of sound, that is, depends upon a suggestion essentially kinaesthetic, of checked or impeded movement, or whether a real auditory harshness is felt. Certain breaths are criticized as hard to finish; *f* and *v*, for example: this is a kinaesthetic unpleasantness. *Sh*, *th*, and *zh* were often condemned as 'sloppy.' This is probably a sound association, a suggestion of the noises made by splashing liquid. That the vowel sounds should have no qualifying adjectives except 'pleasant' and 'unpleasant' is easily understood. There is much more of the purely auditory character and less of the kinaesthetic about a vowel than about a consonant. The long vowels are generally preferred to the short ones (although short *e* is a very popular vowel) because they are more musical; the tone quality has more time to be appreciated. Musical changes have an affective value in themselves that

it is difficult to trace to any associative source. Articulatory movements, on the other hand, have the same reasons for being pleasant and unpleasant as any other bodily movements; they are agreeable when they are free and unconstrained, disagreeable when they are the reverse. The indications from our experiments are that the only elements in articulate sounds that do not derive their pleasant or unpleasant character from association, either with meanings or with kinaesthesia, are the vowel sounds.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

COMMUNICATED BY E. B. TITCHENER and H. P. WELD

XXIII. ON INTENSIVE AND QUALITATIVE JUDGMENTS OF LIGHT SENSATIONS

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By E. J. GATES

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Since Hering showed the qualitative character of the black-white series, there has been much discussion regarding the intensive attribute of sensations of light. We sought, in the present study, to find out whether an observer is able to compare two such sensations both qualitatively and quantitatively; whether, that is, it is possible to determine both an intensive and a qualitative limen with the same light stimuli; and, if so, whether the two limens coincide.

*Apparatus.* We used the Whipple apparatus for discrimination of brightness.<sup>1</sup> The lamp was a 10 watt, 110 volt, frosted Mazda; observations were taken only during hours when the university current is not subject to rapid fluctuation. The reflectors were covered evenly with a blue paper (Milton Bradley blue tint no. 2). The observer sat with his head in a head-rest, and with his eyes distant 50 cm. from the windows of the apparatus. Exposure was made by means of a shutter; the regular exposure was 5 sec.; but if judgment was passed within that limit of time, the shutter was immediately closed. The work was done in a dark room, with adaptation to dark.

*Method.* The method of limits, as set forth in Titchener's Manual, was followed for the determination of a lower *DL*. The normal stimulus was given by the setting of the pointer at 0° of the scale (high but not maximal illumination); the unit of change for the stimulus of comparison was 2° of the scale. The experiment was so planned that the ascending and descending series, the right and left positions of the normal stimulus, and the instructions for intensive and qualitative judgments, were evenly distributed. In all, 64 series were taken from each observer. Preliminary practice was given, under definite instruction, in order that a standard of judgment might be attained.

*Observers and Instruction.* The observers were Dr. E. G. Boring (B) and Mr. G. J. Rich (R). The former was highly practised, and had a full theoretical knowledge of the question at issue; the latter was relatively unpractised, and knew no more of the problem than he had learned in a course of lectures on systematic psychology. At the beginning of every series, the observers were told that it would

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<sup>1</sup> G. M. Whipple, Manual of Mental and Physical Tests, 1910, 163; i., 1914, 197.



be an ascending or a descending series, and were instructed to judge in terms of intensity (by 'greater,' 'equal,' 'less') or in terms of quality (by 'lighter,' 'same,' 'darker'). No introspective reports were required during the experiment; but at its close the observers were asked to distinguish, as accurately as they could, between the intensive and qualitative judgments and attitudes.

*Results.* The numerical results were as follows; the units are degrees upon the scale of the apparatus:

	Observer B	Observer R
Qualitative <i>DL</i> .....	25.1 ± 4.1	27.6 ± 2.7
Intensive <i>DL</i> .....	22.2 ± 4.6	24.6 ± 3.1
Qual. <i>DL</i> ascending.....	23.5 ± 3.8	26.4 ± 2.3
Qual. <i>DL</i> descending.....	28.7 ± 4.2	28.9 ± 2.8
Int. <i>DL</i> ascending.....	19.4 ± 4.0	24.1 ± 3.9
Int. <i>DL</i> descending.....	25.1 ± 3.2	25.1 ± 2.3
Qual. space error.....	—3.4	—1.6
Int. space error.....	—3.1	—1.6

The probabilities that the difference between the qualitative and the intensive *DL* (2.9, 3.0) is significant are, in the case of B, nearly 100:1 and, in the case of R, enormously greater.

*Discussion.* It is clear that the two observers have approximately the same *DL*; and it is clear, from the trend of the figures, that they adopted similar attitudes to the stimuli. The reports appear to show (1) that the instruction for quality brought out ordinarily pure qualitative judgments, but (2) that the instruction for intensity brought out judgments of insistence or *Eindringlichkeit* and not of intensity proper. Observer B writes:

"In judging 'intensity' I tried to give myself up to the quantitative aspect of the experience, to take it with regard to the 'total force of the impression.' The judgment was generally in terms of relevant kinaesthesia; the one which came nearer to 'knocking me down,' as it were, was taken as the more intense. Quality was judged as 'degree of blackness.' I do not know whether I could have done my series in terms of white, i. e., taken the ascending series as meaning 'more white' and the descending as 'less white,' but it does not seem to me now that I could. The judgment 'lighter' was particularly hard to give, and in those descending series where I did give it, I actually shifted the basis of judgment. . . . I was early aware that the qualitative limen seemed to be greater than the intensive, and this fact may have influenced my judgments. I was generally aware of both changes in a given series; and in the descending qualitative series would not expect a change until the intensive change had appeared; contrariwise in the ascending intensive series. I was less certain of the qualitative judgments than of the intensive. This was due, I believe, to the fact that the equality of the intensive series meant more nearly a total identity than did the 'same' of the qualitative series. It is always easier to judge of identity of two impressions in respect to all their attributes (cf. color-mixing) than

to judge of identity of a single pair of attributes when the others differ."

B, then, judged 'intensity' by total impression or insistence, and naturally found these judgments easier than the qualitative.<sup>2</sup> R writes, less analytically:

"I had considerable difficulty in getting criteria of difference between quality and intensity. I presently came to take intensity, not as intensity of visual sensation, but as intensity of this whitish sensation; however, it was always more or less of a qualitative thing. As the variable slit was changed from sameness or equality, the first difference I noticed was what I reported as a difference in intensity. It came in very gradually, but was not difficult to judge. A little further along there was another change, and this I reported as difference in quality; towards the end of the experiment I was surer of it."

In the light of B's report, it seems permissible to say that R, too, judged 'intensity' by total impression or insistence.

Referring now to the numerical results, we see that the intensive limens are in fact significantly smaller than the qualitative limens which correspond with them; though the subjective assurance of the observers is not reflected objectively in the values of the *MV*. It is noteworthy, however, that the two series which are mentioned by B as involving the dual criterion (the descending qualitative and the ascending intensive) give the extreme liminal values: the descending qualitative goes lower than its pair towards black, and the ascending intensive goes higher towards white.

*Remarks on Method.* We made various attempts (gelatines, tissues) to eliminate the yellowish quality of the stimuli, and finally had recourse to the blue paper spread on the reflectors. In the preliminary series this paper was judged to be effective. In the experiment itself, R did not notice color; but B writes: "The stimuli were noticeably yellow and seemed more so at some times than at others. I constantly and consciously abstracted from the color, which was bothersome, however, only in the intensity series." If the experiment is repeated, the yellow should be more adequately compensated.

The exposure-time of 5 sec. is probably too long. The observers knew their method, and realized that they were to give immediate judgments. In cases of difficulty, however, they utilized the full 5 sec. B writes: "I was bothered at times by the fear that I was considering an extensive difference in the qualitative series; for in cases of long exposure the 'blackness' seemed occasionally to appear in patches. I am not sure of this, however." Care was taken, of course, that there should be no interference by after-images; but an exposure-time of 3 sec. would probably have simplified the experiment. We chose the longer time with the idea that the judgments would thereby gain a surer and more definite basis.

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<sup>2</sup> Cf. the ease of judgment reported by L. G. Meads, this JOURNAL, xxvi., 1915, 151. As B observed in this study, and may therefore be thought to have been influenced by suggestion in the present work, it is well to state that our series were completed before Meads' were begun.

*Conclusion.* We conclude, so far as this study goes, that 'intensive' judgments of light sensations are likely to be founded upon insistence or 'power to catch the attention' rather than upon intensity itself. We think it possible that some of the extant determinations of the *DL* for 'brightness' are measures of this insistence rather than of intensity or quality. Whether it is possible by more rigorous instruction, inducing a conscious abstraction from other attributes, to secure a true intensive *DL* of light sensation remains an open question.

## BOOK REVIEWS

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*Il metodo delgi equivalenti. Contributo allo studio dei processi di confronto.* By AGOSTINO GEMELLI. Firenze, 1914. Libreria Editrice Fiorentina. pp. 344.

This monograph is an attempt to work out experimentally, in the field of the comparison of cutaneous distances for different parts of the body, those factors that may influence the method of equivalents. The treatment naturally falls into three parts:—1. an experimental attack upon the factors involved in the method from the purely quantitative side; 2. a statistical attack upon the influence of the factor of the use of visual imagery in making the judgments; and 3. an attempt at an introspective analysis from the qualitative side of the comparison judgment under his experimental conditions. The work was prepared under the guidance of Professors Kiesow and Külpe and clearly shows the influence of both men.

Gemelli invented a new aesthesiometer for his experiments, which not only has the advantage of being exceedingly handy and usable, but also,—and this is of much greater importance,—which enables one to control the intensity of the stimulation as well as the distance between the points. In his experiments, Gemelli employed that technique of the method of equivalents which has become known as procedure G<sub>1</sub>. The following parts of the body were stimulated in successive fashion:

1. forehead—lower arm;
2. lower arm—chest;
3. lower arm—back;
4. forehead—chest.

The standard distances varied from threshold values to 12 cm. The ratios of the subjective equivalents to the objective values of the standards are found to be very similar to those obtained by Miss Washburn. These ratios set in with very high values and decrease as the extent of the standard increases. This decrease is found to be constant up to standards of about 8 cm. and then the values of the ratios increase again. The value of this ratio also depends on the sensitivity of those parts of the body that are stimulated, and it is found that the ratio becomes larger the greater the difference in sensitivity of the two points stimulated. This Gemelli believes is an index that the ratio increases along with the difficulties that present themselves to the subject in comparing the two distances. Another factor studied is the effect of the intensity of the stimulation or the amount of pressure exerted by the stimulus. It is found that as the pressure is increased the ratio more nearly approaches unity. Hence Gemelli studies experimentally various factors that may influence the results of this method in the tactual field. He makes no effort to study those formal considerations which constitute the basis of the method, nor does he attempt any consideration of the formal relations between this and the other psychophysical methods.

Gemelli now attempts to determine statistically the effect of the

presence or absence of visual images in a comparison judgment under these conditions. This is studied by determining the ratios for sensations on the forehead and on the arm held in different positions. In one group of experiments the arm is held along the body, while in the other it is abducted as far as possible. It is found that cutaneous distances on the arm are underestimated when that member is extended, as compared with judgments when the arm is held in the normal position. Thus the results show very clearly that distances on any part of the body are underestimated when that part is removed as far as possible from the medium line. These differences Gemelli believes to be due to the presence of visual images when the part is abducted. This contention he attempts to prove by experimentation upon two congenitally blind subjects, who cannot, in the nature of things, make use of visual imagery. For these blind subjects, no such differences in underestimation are to be noted with the arm in the two positions.

In the third part of the monograph, Gemelli attempts an introspective analysis of the comparison judgment under his experimental conditions. For this purpose he employs the method of systematic experimental introspection as advanced by Külpe. Such a comparison judgment is considered to be a thought process and hence is capable of analysis by this experimental method. The single process of comparison reduces to the following steps:

1. Preparation of the subject to the process of comparison;
2. Apperception of the standard stimulus;
3. Pause;
4. Apperception of the comparison stimulus;
5. Formation of the judgment;
6. Expressing of the judgment.

Each of these steps is then analysed; and in regard to the first, the preparation of the subject to the comparison process, the matters of *Aufgabe* and *Einstellung* are discussed. The factors that form the basis of the judgment are found to be of two general sorts. 1. Mediating procedures, which make use of either, a. visual imagery; b. kinesthetic imagery; or c. the use of categories. This use of categories is obviously the passing of an absolute judgment upon each of the cutaneous distances. Besides these we have 2. immediate procedures which include, a. successive observation of the two distances; or b. relative observations of the two distances. The immediate procedures are utilized by the subjects when the differences between the two distances are very great and hence the judgment is comparatively easy. When the differences between the two distances that are to be compared are very small, the mediating procedures are brought into play.

This monograph shows some very careful work on the part of the experimenter and is of great value for two reasons. In the first place, it has renewed the interest in the method of equivalents. This method, in its present form, does not give a determination of the measure of sensitivity of the subject, but on the other hand, from it we perhaps get a better understanding of the meaning of the point of subjective equality than from any of the other psychophysical methods. Unfortunately Gemelli does not attempt to give a formal analysis of the method, but his analysis of the experimental factors is of the greatest value. In the second place, the work is of great value because Gemelli has attempted an analysis of the comparison consciousness from the qualitative side. All of the psychophysical methods stand sorely in need of such an analysis. Whether the reader agrees or dis-

agrees with the analysis made by Gemelli, does not affect the fact that such an attempt is a step decidedly in the right direction. It would appear to the reviewer that Gemelli has made a more complete analysis of the particular consciousness under observation than has yet been done in any of the Würzburg studies. And this analysis has been made from a purely psychological attitude toward the problem and without any appeal to philosophy. Unfortunately, it seems to the reviewer, that Gemelli does not publish sufficient introspections to confirm his analysis.

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*The Influence of Distractions on the Formation of Judgments in Lifted Weight Experiments.* By DAVID MITCHELL, Ph. D. The Psychological Monographs, Vol. XVII, No. 3 (Whole No. 74), 1914. pp. 58.

On the basis of introspection most psychologists have divided attention into at least two sorts—voluntary and involuntary. It is the purpose of this investigation to induce two states of mind which may be characterized by these terms and to study the influence of each upon the formation of judgments. The experiments were carried on in the field of lifted weights. The space errors were eliminated and the weights were presented in the first time order. Two types of sound distractions were employed: 1. a continuous sound made by an electric buzzer to which the subject was instructed not to attend, and 2. the counting of separate clicks—from one to six in number—simultaneously with the lifting of the comparison stimulus. "In the first group the distraction was produced by a sensory stimulation without a concomitant mental activity otherwise induced. In the second group, the work of counting the clicks was a prominent feature of the mental process involved" (p. 33). That Mitchell succeeded in inducing the desired states of mind by means of these distractions is attested by the reports of the observers.

The apparatus employed for the production of the distractions was rather complicated but exceedingly clever and efficient. It was necessary to have the distracting sound exactly, or almost exactly, concomitant with the lifting of the proper weight. This the experimenter obviously could not do. So Mitchell devised a means by which the actual lifting of the weight mechanically actuated the distracting stimulus. Hence the differences in time between the lifting of the weight and the starting of the distraction were exceedingly small.

The experiments were so arranged that normal series without distractions were mingled with the distraction series of both types and so were taken under the same objective and subjective conditions. Four subjects were employed in this investigation and in all over 75,000 individual judgments were recorded; a number amply sufficient to give authority to the results. The data was collected in accordance with and subjected to the calculations of the method of constant stimuli as developed by Urban.

The results show that, contrary to the traditional view, a distraction such as those employed in this experiment tends to produce greater precision of judgment; or in other words, in the presence of a distraction the judgments of the subject are more consistent. This is shown by the greater size of the value of  $h$  of the  $\Phi$  ( $\gamma$ ) hypothesis for the series in which the distractions were present. Also this presence of distractions increases the sensitivity of the subject. The

interval of uncertainty is much smaller for the series in which the distractions were present than it is where there were no distractions. This influence is seen to be present upon both of the thresholds in the directions of increase and decrease. *i. e.*, both thresholds tend to more nearly approach one another. The influence is greater, however, on the threshold in the direction of increase. These results are in accord with those from the Cornell Laboratory of a few years ago. As a result of this greater influence upon the upper threshold, the point of subjective equality is shifted and assumes a slightly lower value for the series with distractions. Hence the conclusion is drawn that the presence of a distraction causes an overestimation of the weight. This is in contradiction with the traditional view which holds, to express it in positive terms, that an increase of attention tends to increase the intensity of the sensation. This shift of the point of subjective equality is relatively small however.

Perhaps Mitchell's most striking results are those obtained when we compare the different final values furnished by the distraction series, where the distraction was a continuous sound to which the subject did not attend actively; and the other, where the distraction was the active counting of discrete clicks along with the lifting of the comparison stimulus. It is found that the coefficient of precision, the intervals of uncertainty, and the points of subjective equality for these two series are, on the average, almost identical. Hence we may conclude that both types of attention or distraction had approximately the same effect upon the formation of the judgments. It is difficult to believe that two mental states that are introspectively so apparently different should have such similar effects; but, nevertheless, the results of this investigation show that this is the case. Two alternative hypotheses are advanced in explanation of this difficulty. 1. It is suggested that our introspection may not be one of attention itself, but rather of the conditions of attention, and that the processes of attention may be the same whether voluntarily or involuntarily initiated. 2. It may be that introspection gives us the subjective aspect of two very different sorts of process but that these may be of such a nature that the differences succeed in counteracting one another in such a way as to neutralize themselves and so bring about identical results.

This paper is exceedingly significant inasmuch as it is another successful attempt to extend the application of the psychophysical methods beyond the domain of sense-perception. The paper also opens up a very interesting group of problems that must be solved by future research. Furthermore, it indicates most clearly that an introspective analysis, even though complete, is not the final goal of psychology. Because we must then answer the question as to what is the effect of this combination of structural factors. Mitchell gives his numerical results with great fullness so that his study may be more easily comparable with the results of future investigations. He also includes a number of charts of the curves of the psychometric functions of the different judgments under these different conditions, so that the reader has a graphic presentation of these relationships that is exceedingly helpful.

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*The Psychology of Learning: an Experimental Investigation of the Economy and Technique of Memory.* By E. MEUMANN, Professor of Philosophy at Hamburg. Translated by JOHN WALLACE BAIRD, Clark University. New York, D. Appleton and Co., 1913. x + 393 pp.

This translation of Meumann's *Oekonomie und Technik des Lernens* is based on the third edition of the German work, which appeared in 1912. The present text approximates a complete exposition of the psychology of memory and its application to practical life and education.

In the preface to the American edition Meumann calls attention to the close community of interest and effort between this country and Germany in the fields of experimental psychology and experimental pedagogy. "It may indeed be said that these sciences have been created by the two nations. . . . In both countries there is a deep-rooted conviction that the most important problems of the science of education can be solved only by an appeal to experimental psychology, and by an application of the methods of psychology to the problems of pedagogy."

The scope of the book may be seen from an enumeration of its contents, although only a few of the more general features can be mentioned at any length. The first three chapters, including the introduction, are devoted to a preliminary discussion of learning and especially to a general analysis of the functions of memory. The relations between observation (perception) and memory have been worked out with great care in Chapters III and IV, in the former of which Meumann gives a very complete analysis of the process of observation and in the latter a resumé of experimental investigations of this function, their methods and results, especially as applied to children. The remaining chapters, comprising more than half of the entire volume, deal with Associative Learning; memory types; economical learning; conditions and technique of mechanical learning; stages of learning; retention and forgetting; the education of memory in the schools; etc.

The author's plan in connection with all these topics is first to make a minute and careful psychological analysis of the functions concerned, second to give a resumé of experimental results, and, finally, to apply the evidence thus obtained to the field of pedagogy. The thoroughness with which this purpose is carried out is admirable, and the book is full of helpful practical suggestions. The evaluation of experimental results is especially good, though one might wish in this connection that the author had drawn more freely from American sources at this point. It may be remarked in passing that of the 136 titles given in the bibliography at the end of the book only 24 are of English and American origin.

Besides the bibliography the book contains three appendixes entitled:

- I. The construction of series of non-sense syllables.
- II. The meaning of economy of time and energy in learning.
- III. Addenda from the most recent literature.

In the introductory chapter, Meumann seeks to make clear the meaning of "technique" and "economy" as applied to the learning process. In order to develop such a technique and promote economy of time and effort, it is necessary to consider not only the general conditions of learning but also more special conditions, such as those which depend on the specific purpose of the learning act and those



dependent on the *will* of the learner. It is worthy of note that a great deal of stress is made at this point and throughout the work on the importance of the will, though no attempt is made to analyze or describe this important factor. "The success of every mental activity, including the act of observing, depends not only upon the accomplishment of the component activities but chiefly upon the co-operation of the will. Will determines how much energy and persistence shall be devoted to the act of observing . . ." (p. 64.) And again, "We spur and impel the will from within whenever during the course of a long act of learning we detect that the attention is flagging . . ." (p. 283.) And yet again, "We profit from continued practise only in proportion as we incite the will to progress and arouse an intention to improve." (p. 360.)

In Chapter I, on the Modern Psychology of Memory, we come upon the author's view of general training which is the underlying doctrine of the book. Whenever training in memorizing takes place a two-fold result occurs: (1) the particular material or content is imprinted on consciousness and is thereafter capable of reproduction; (2) the "memory itself, i. e., our general retentive and reproductive capacity, is strengthened." (p. 6.) It is true that Meumann admits later that there is no such thing as "general memory," but the term conveniently expresses the fact that in every act of memory there is a formal training consisting in the increased capacity to acquire practice-dispositions and of setting attention and will to work in the service of memory. Furthermore, the term "general memory" is significant because of the fact that in all cases of the memorizing of special contents improvement takes place in other special memories more or less closely related to the function trained. The well-known results of the experimental work of Ebert and Meumann, on which the latter conclusion is mainly based, are reproduced in detail in a later chapter of the book, but no consideration is given to the divergency of results obtained by Dearborn and other investigators.

The practical outcome of this view is the emphasis on the importance of formal training. "Laboratory investigations of the development of the mental functions of adults show us what an enormous loss is entailed in modern education as a result of its prevailing emphasis upon content as the essential aim in teaching. In view of the fact that the students in our universities must be taught to see correctly and to hear correctly, that they scarcely know what their mechanical memory is capable of accomplishing, that accuracy and fidelity of description are not developed until they come to our laboratories for experiments, that elementary differences of endowment are levelled down so little during the first twenty years of their lives,—in view of all these facts, it appears that our modern system of education fails to fulfill the demands both of science and of practical life. . . . The memory of modern man is wholly neglected, in so far as its formal aspect is concerned; even an improper memory training is better than none at all." (p. 186.)

Another outstanding feature among the more general features of the book is Meumann's stress on the doctrine of types. We have types of attention, of association, of learning, of observation, and of memory and ideation. Individual differences in memory and ideation are fundamental in any account of the learning process and constitute the basis of the psychology of memory and the pedagogy of learning. Thus the typically slow learner is usually of the visual ideational type, though slow learning may also result from slow adaptation of

the learner's attention; the auditory-motor type of memorizer is more rapid, though less sure in learning. All-round development of imagery is the educational ideal. Meumann believes that these types are the result of congenital dispositions but that they are subject, though to a relatively slight degree, to the influence of training and education. Teachers should examine children with a view to discovering their memory types and also their attention types and the essential features of their methods of learning should be brought to the pupils' attention. Formal memory exercises should at first be adapted to the peculiar mental types of the children, but gradually there should be a transition to the development of an all-round training of imagery.

It is fortunate that this work on the learning process by the leading exponent of experimental education in Germany has been made accessible to the wide circle of American readers who are interested in this important field. Professor Baird has succeeded in rendering the work into English of clear and readable character—a task which those who have read the original will agree was no easy one. The detailed analyses, classifications and sub-classifications, added to the somewhat prolix (from an English point of view) style of the author, have not prevented the translator from giving us a work easily understandable by anyone familiar with psychological literature. At the same time the original has been faithfully followed and we now have for the first time in English a résumé of Meumann's views on education.

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E. H. CAMERON

*Grundzüge der Psychotechnik*, von HUGO MÜNSTERBERG, Leipzig, 1914. Verlag von Johann Ambrosius Barth. 733 pp. Bibliography.

The *Grundzüge der Psychotechnik* was first formulated as a series of lectures at the University of Berlin—the first university to offer a course in Applied Psychology, the author tells us. It is dedicated to James McKeen Cattell. The principles laid down in Volume I, published many years ago, and which were worked out without especial changes in other publications, appear now in this—Volume VI—in their true meaning and justification. For according to the belief of the author, an investigation, scientific or otherwise, is of value in proportion as it contributes directly to the common good.

As in the case of other German publications of Münsterberg, this is a systemic treatise, written in textbook style, dogmatic in tone, with much attention to definition, distinction and illustration—but few facts. Indeed, it seems to be the view of the author that facts are relative and negligible—the theory's the thing. And this presentation of the theory is directed to man in his administrative capacity, as the following may indicate "If we are to change the world, to reform and improve men, to teach or to cure them, to make them perform efficient labor or to organize them for common action, then we must treat man as a system of causes which will produce certain effects. We must be able to foresee what will happen and to determine how we can mold the mind." This quotation taken from a corresponding English book by the author, represents fairly, I think, the author's point of view. It is the parent-child relation looked at from the parent's point of view. It is the individual to be manipulated by the administrator, not the individual from his own point of view which is investigated. The practical administrator, then, is the object of Professor Münsterberg's regard—the jurist, the physician, the preacher, the politician, the man of commerce. The book is in-

tended as an introduction to the psychology of his subject, for each of the men named. Suggestion therefore plays a considerable part in the discussions, and the hope of acquiring facility in the art of suggestion for practical ends is the motive to which the book seems to appeal.

But the author hopes that Psychology also will reap some reward. In recent years considerable numbers of men of zeal but little training have undertaken investigations of the individual in his relations to the fields of education, sociology, medicine, ethics, economics, politics, etc. Also, the men actively engaged in these fields look to Psychology for light which they do not find. And furthermore, some Psychologists have gone over into practical fields and have found the problems of real life. While such efforts are praiseworthy, there is a vast waste of effort owing to the vagueness of the fields and the lack of a single point of view; and results of the labor are often useless for want of fundamental principles. These fields of work then, so far as they are concerned with the individual in his mental aspect, need their problems more clearly formulated, legitimate aims set in the foreground, and the fundamental principles so definitely pronounced that order may come of chaos. Under such an Aufgabe, further work might be stimulated and the results become of value, whether this work is done by psychologists, pseudo-psychologists or the men actively engaged in such fields. To supply the deficiency of aim, problems and principles is the purpose of this volume, and it is hoped that Psychology may benefit thereby.

The volume is divided into two parts, of which Part I deals mainly with the implications of the double standpoint in Psychology, its hypotheses and the relations to Applied Psychology. Over against Applied Psychology, we have Theoretical Psychology, which may be subdivided into Causal and Purposive Psychology. The general argument is that Causal Psychology has become too narrow and unproductive; that it has set for itself unnatural and artificial problems; that it has become limited almost to Physiological Psychology. Causal Psychology should have a wider and more useful aim. It should consider, for example, the influences which surround each individual—such as suggestion from without—and influences which are internal—such as autosuggestion, voluntary attention and will. Present day Experimental Psychology, in limiting itself to the content of consciousness, cannot find these influences because they have no content. Nevertheless, the author believes, they are vital forces in our lives.

Part II deals with the special fields of Sociology, Personal and Community Health, Economics, Ethics both social and individual, Education, Art, and finally the relations of Psychology to the Natural Sciences, Historical Sciences, Philology and Philosophy. The rising appreciation of wide individual differences in society, of radical, conservative and socialistic tendencies, of race mixtures, of sex differences, of the young and aged, renders it unnecessary to argue at length as to the need for a science of psychognosis. In the discussion of each of the above fields, the points at which such a psychognosis is needed, are indicated. The advice to the sociologist (or to any one in another field) is usually phrased as follows "Psychology can help here," "This problem cannot be solved without the assistance of Psychology," etc. In similar fashion, the general problems of each field are put under the microscope and are seen to have a psychological aspect and a technical aspect. Neglect of factual material, however, leave the reader somewhat unconvinced as to the exact contribution which

Psychology, as distinguished from Psychognosis can make, and the respects in which the Psychognosis proposed will be superior to that which the Historian or Economist now make. And while the author claims the prime necessity of scientific method for the solution of these problems, it is evidently not the method with which Experimental Psychology is familiar. For the practical man is interested in ends, purposes, personality, Will—in short, in an interpretation of life. And for this an Applied Psychology is needed. Such a Psychology would perhaps be willing to recognize a certain small value in Experimental Psychology of the present day variety—provided Experimental Psychology will lend itself a little more to this view of the necessity of interpreting life—but otherwise, both Theoretical and Applied Psychology can and will neglect Experimental Psychology and proceed with their own true business. Throughout, Professor Münsterberg pays his respects to Experimental Psychology in this vein.

References to experiments already performed by Professor Münsterberg in the field of Applied Psychology, are somewhat vague. "Experience shows," "the analysis shows," "the experiments indicate" are frequently repeated. Experimental psychologists who wish that these experiments had been carried at least to the point of control, and that hypotheses had come from described facts, find therein a meaning of the term "experiment" which it seems highly desirable to avoid. The purpose of an experiment, in the sense in which it appears in this and other writings, seems to be—to convince the experimenter that his previous judgment is correct. But if the conclusions are disputed, there are no records which will show that this conclusion and no other could have been drawn from them. For Münsterberg, an experiment has served its whole purpose when it offers an interpretation to the experimenter. For Applied Psychology, the experiment should yield a suggestion for some situation in practical life. The suggestions may then be ordered, classified, and finally articulated in terms of the theory which preceded the experiment. Following this method, Münsterberg has undoubtedly done his work in masterly fashion. It is a logical, well organized and well balanced presentation. Perhaps the only criticism which Experimental Psychology should offer is that it leaves room for doubt whether or not there are any facts to support the conclusions.

A good working bibliography for each of the applied fields is to be found at the end of the book.

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A. H. SUTHERLAND.

*The Negro Races: A Sociological Study.* By JEROME DOWD. II, New York, The Neale Pub. Co., 1914. 310 pp.

This volume is one of a series on the sociological study of mankind from the standpoint of race. The author is head of the Department of Economics and Sociology in the University of Oklahoma. The first volume, which dealt with the West Africans, was published by the Macmillan Co. in 1907. The present volume is devoted to the Negroes of East and South Africa. A third volume, on the Negroes in America, is in course of preparation.

In the preface to the earlier volume, the author points out the fact that "up to the present time sociologists, in tracing the evolution of society, have constructed theories based upon data selected promiscuously from opposite quarters of the earth and from many different races." This method would suffice if the races had lived

in the same environment and had undergone the same stages of development, which they have not. The first object of the present work is "to establish the fact that each race has its distinctive institutions and special evolution corresponding to the locality in which it lives or has lived." His criticism of the abuse of the "comparative method" by so many writers and his emphasis on the importance of regional studies of human culture are to be commended.

Stress is laid upon the influence of the physical environment, because "that factor is always predominant in the early stage of development, and only diminishes gradually as man strengthens his intellect and adds to his knowledge. This environment first controls man, after which man controls the environment." The last sentence is perhaps a rather too general and antithetical statement of an actual difference between primitive life and civilization with respect to the relations of man and his physical environment.

The Negro territory is divided into a number of geographic areas, such as the cattle zone, the forest zone, the banana zone, the millet zone, the camel zone, etc., each of which is given separate treatment, followed by comparative surveys. In each zone, the several phases of native culture are studied, including the economic, domestic, political, religious, and aesthetic life, customs and ceremonies, and psychological characteristics. The sources from which the author draws his data are mainly authoritative writings by first-hand observers. Secondary sources like Ratzel's "History of Mankind" and Reclus' "The Earth and its Inhabitants" are also used, and rather too much reliance, it would seem, is placed on such popular compilations as "The Living Races of Mankind."

The influence of the physical environment on those complexes of ideas and habits which we call culture is certainly of great importance, and Professor Dowd has rendered a notable service in showing the operation of this influence among the natives of Negroland. He is on more doubtful ground in his views of the relation of environment to bodily traits, more particularly cranial capacity, and of the relation of the size and form of the head to mental characters. A correspondence between size of brain and intelligence, either in groups of people or in individuals (within certain normal limits) is far from proved; indeed the evidence appears to be largely negative. Also such theories as the one, quoted with apparent approval, that "the dolichocephalic type is everywhere more domineering and ambitious, and is generally represented among the higher and ruling classes and is more largely represented in cities" (p. 95) cannot be accepted. In the opinion of the reviewer, the evidence is scarcely sufficient to substantiate a belief in "a correspondence in all the African zones between the size and form of the brain and its activity." Also we are inclined to think too much faith is placed in natural selection as a modifier of brain capacity. Professor Dowd seems to accept the extreme views of Major Woodruff on the influence of geographic conditions on the brain. According to this view, the general trend of increase of brain is away from the tropics; in a tropical country where existence was easy, a large brain could not evolve. Of course this view is connected with the other, that a large brain means high degree of intelligence. The author's position is summed up in his words, "The process of natural selection adjusts the brain capacity to the conditions, and the process is rapid." These views should be revised in the light of such facts and arguments as are found in writings like Boas' "Mind of Primitive Man" and Wissler's article

in the *Journal of Religious Psychology* for July, 1913, on "The Doctrine of Evolution and Anthropology."

The following quotations will further illustrate the author's conclusions. "The Negro races respond to environment in the different zones of Africa just as the Caucasian and Mongolian races form different types in their respective localities. If there is a difference of races in plasticity and responsiveness to external phenomena it is probably in favor of the Negro. There are probably greater diversities of the Negro in Africa than of the Caucasian in Europe and America." "The backwardness of the Negro in Africa is not due directly to lack of mental capacity but to unfavorable environment." (The author must refer here to the initial or potential capacity of the original stock, for he holds that the environment has affected the present hereditary powers of the brain). "If any other race had peopled Africa in early neolithic times, and remained there until now, it would have advanced no higher than the present culture level of Negro." (p. 277.)

In answer to the question whether the Negro could, under a favorable environment, develop to the same degree of culture as the Caucasian and the Mongolian, the reply is that "in view of the known modifiability of races, it seems reasonable to assume that the Negro would be thoroughly capable of evolving a civilization, but such a result would require a long process of natural selection, and freedom from the antagonistic influences of the more fortunate races."

Professor Dowd holds that the most conspicuous mental trait of the Negro is "excessive emotionalism." We will just refer to the experiments by Miss Keller in Chicago which showed negroes to be decidedly more stolid than white women, as regards expression of emotion. We also call attention to these words of Dr. Marett of Oxford: "It may well be that it is not the hereditary temperament of the Negro, so much as the habit, which he shares with other people at the same level of culture, of living and acting in a crowd, that accounts for his apparent excitability." (*Anthropology*, p. 92.) According to Professor Dowd, "races, as individuals, have inherited emotional characteristics, and these must always color the products of their intellects." Also "the instincts of the Negro differ from those of the Caucasian in intensity and direction. Under changed conditions they would, no doubt, be greatly modified, but they would never behave in the same way as those of the Caucasian." (p. 279.) For an excellent criticism of the view that there is an innate equipment for the acquirement of a particular culture, see the above mentioned article by Dr. Wissler.

University of Minnesota.

ALBERT N. GILBERTSON

## BOOK NOTES

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*On the cosmic relations.* By HENRY HOLT. Boston, Houghton Mifflin Company, 1914. 2 v.

These volumes attempt a comprehensive philosophy. The evolution of the body and the soul and of the universe are first sketched, and the relations between the known and the unknown in the universe are discussed together with the ethical aspects of evolution. This is termed correlated knowledge. The second book, on uncorrelated knowledge, describes telekinesis, molar and molecular, and the same distinctions are applied to telepsychic telekinesis. Then comes auto- and psycho-kinesis. A very long part is devoted to telepsychosis, in which the cosmic soul, the idea, possession in general, heteromatic writing, dramatic possession, Mrs. Piper, the various Hodgson and Newbold reports, Hyslop, the heteromatic script and various other sittings are treated. The third book includes attempts at correlation, the relation between mediumistic and other dreams, the making of a medium, the dream life, pros and cons of spiritism, dreams indicating the survival of death. The book has an admirable index.

*Mental medicine and nursing.* By ROBERT HOWLAND CHASE. Philadelphia, J. B. Lippincott Company, (c. 1914). 244 p.

This handbook is meant simply as an introduction to the study of mental diseases. The first part treats of the nervous system, neurons and tracts; part two, of apperception, feeling, emotion, sensation, complexes; part three, of insanity, its general symptoms, disturbance of mental elaboration, of the feelings and emotions, will and acts; part four, of mental diseases, auto-intoxication psychoses, psychoses due to organic cerebral affections, and constitutional psychopathic states; while the fifth and last part discusses the patient from the physician's point of view, and nursing, feeding, etc. The book contains fifty-seven cuts.

*The individual delinquent; a text-book of diagnosis and prognosis for all concerned in understanding offenders.* By WILLIAM HEALY. Boston, Little, Brown & Co., 1915. 830 p.

The author has been for years the director of the Psychopathic Institute of the Juvenile Court in Chicago. The chapter heads are orientations, the individual, mental basis of delinquency, working methods, including psychology, statistics, with methodology, conclusions and theories of treatment. Book two is devoted to cases, types and causative factors, under which are discussed heredity, developmental and physical conditions, peculiarities and ailments, physical anomalies, stimulants and narcotics, environmental factors, professional criminalism, mental imagery, habit, conflicts, abnormal sexualism, epilepsy, mental defect, subnormality, aberration, adolescence, its peculiarities, pathological lying, love of excitement, stealing.

*Genetic theory of reality, being the outcome of genetic logic as issuing in the aesthetic theory of reality called pancalism.* By JAMES MARK BALDWIN. New York, G. P. Putnam's Sons, 1915. 335 p.

Under genetic interpretations we have first the problem and genetic morphology, then the individual interpretation, the parallel between it and the racial one. Part second treats the development of interpretation and stresses the prelogical character of the early racial interpretation, also its positive character, the religious interpretation, religious reality and negation, logical interpretation, with mediation theories, especially those based on the primitive and transcendent, and the immediacy of synthesis, the demand for an intrinsic one. Part three is on aesthetic immediacy, and deals with the intrinsic synthesis of aesthetics, its interpretation. The conclusions advocate what the author dubs pancalism, which is a theory of reality from which he deduces various corollaries.

*Educational psychology; briefer course.* By EDWARD L. THORNDIKE. New York, Teachers College, Columbia University, 1914. 442 p.

In this work the author has rather simplified for beginners his three previous volumes, "The Original Nature of Man," "The Psychology of Learning," and "Individual Differences and Their Causes." It is intended as a text for colleges and normal schools. First come the general characteristics of original tendencies, then man's equipment of instincts and capacities, original satisfiers and annoyers, tendencies to move, learning capacity, anatomy and physiology of original tendencies, order and dates of appearance and disappearance of original tendencies, and value and use of them. Part second is the psychology of learning; part three, individual differences and their causes.

*The psychological researches of James McKeen Cattell; a review by some of his pupils.* (Archives of Psychology, No. 30, April, 1914.) New York, Science Press, 1914. 101 p.

The pupils of Professor Cattell, wishing to recognize in some public way the completion of twenty-five years of service to psychology, have attempted to review and digest his work and to develop still further certain lines in which he was a pioneer. The volume, therefore, not only presents in an admirable light the work of a devoted student of psychology but it also bears incidental witness to the high esteem and affection in which Dr. Cattell is held by his pupils.

*Scopolamine-morphine anaesthesia.* By BERTHA VAN HOESSEN. And *A psychological study of "Twilight Sleep" made by the Giessen method.* By ELISABETH ROSS SHAW. Chicago, House of Manz, (c. 1915). 216 p.

These chapters discuss pharmacology, toxicology and physiological action of scopolamine, its administration for surgical and especially obstetric anaesthesia, typical and atypical cases, report of 5,000 anaesthetics, report of 100 consecutive cases of twilight sleep, mental effects of it.



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## AN EXPERIMENTAL ANALYSIS OF THE PROCESS OF RECOGNIZING<sup>1</sup>

By ELIZABETH L. WOODS, PH. D., Vassar College

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<sup>1</sup> From the Psychological Laboratory of Clark University.

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## I. INTRODUCTORY

This study is concerned with an analysis and description of the process of recognizing. When the preliminary work was begun, the question in the investigator's mind was this: What is the content of the recognitive consciousness? As the investigation proceeded and it became amply evident that the content of the recognitive process, instead of being unique and constant in its character, may vary widely both qualitatively and quantitatively, the problems formulated themselves as follows: What are the mental components which may function as the content of an act of recognition? What is the nature of the functioning of such contents, *i. e.*, in what order do they appear? Is the awareness of familiarity influenced by the relative intensity and duration of different mental events composing the content of the act of recognizing? Is there a difference in the nature of the content, or of its functioning or of both, in the recognizing of relatively novel and of exceedingly familiar stimuli? What is the essential characteristic which gives to the consciousness of familiarity its unique and proper flavor? These are the problems which this investigation was finally designed to attack.

It is a pleasure to acknowledge my debt both to Professor J. W. Baird, who suggested this problem and who has given generously of his time as adviser and as observer, and to the other observers, upon whose faithful work this study is based.

## II. HISTORICAL

The experimental history of the analysis of the process of recognizing may be said to begin with the Lehmann-Höfding controversy<sup>2</sup> which was opened in 1889 by an article in which Lehmann (25) maintained that recognition is due to association by contiguity. He pointed out that every perception is attended by a number of associated images, and he held that when a given perception is re-experienced, the images formerly associated with it are incited to arousal, the result of such incitement being the experience of familiarity. Lehmann (25 and 26) offered evidence which, he believed, supported this view. He asked seven observers to report whether or not a given odor was known to them, and then to give an account of all the images which were present to consciousness after the presentation of the stimulus. In his four hundred and twenty-eight cases he obtained 84.7 per cent. of recognitions, and 27.6 per cent. of these recognitions were attended by images of the names of the stimuli; 44.9 per cent. were attended by other instantaneously reproduced

<sup>2</sup> E. H. Weber's experiment with visual lengths and weights (*Der Tastsinn und das Gemeingefühl*, 1851) was not concerned with the nature of the process of recognizing.

images; 5.2 per cent. by images subsequently appearing, and 7 per cent. by no images whatsoever. Lehmann explained the latter cases on the supposition that images had appeared but in such dim and transitory form that they escaped the attention of the observer. Furthermore, he maintained that it was the inciting of associated images, rather than their actual appearance, which gave the characteristic quality of 'known' to any stimulus.

Höfding (35) granted that associations are frequently present in the recognition of objects, but he denied that they are essential to the act of recognizing, for example, to the immediate recognition of a very familiar object. He asserted, on the contrary, that the perception of familiar objects is qualitatively different from the perception of novel objects; and the quality which is immediately present in the former case, he calls the *Bekanntheitsqualität*. This quality of 'known,' Höfding believes to be due to a fusion of the perception with an immediately aroused image of itself,—the fusion differing from an association in that a unitary experience results. This *Bekanntheitsqualität* is to be referred to a physiological basis,—an increased facility of nerve functioning.

Lehmann denies the existence of Höfding's 'immediate' recognition, basing his case on a series of experiments in which he employed different shades of gray. His procedure consisted in displaying a gray momentarily and after a short interval in displaying another. He then asked the observer to determine whether the second gray was or was not identical with the first, and to give an account of the ideas and other contents which had attended his determination. Again, Lehmann found no case of recognition which he believed to be devoid of reproduced images; and again he urges that the inciting of associated images constitutes the essence of recognition.

Höfding (36) answers this article by maintaining that 1. Lehmann's exposures were made at such short intervals that the 'disposition' set up by the exposure of one gray, if not the image itself, must still be functioning at the time of the second exposure; and 2. the artificial conditions of the laboratory render the normal cognitive process impossible.

Bentley (42) attempted to determine the status of the image in the functioning of memory. In extended experiments upon visual imagery, he collected evidence which strongly indicates that assured and correct recognitions frequently occur in cases where no image is present. Whipple (23) reached the same conclusion as a result of experiments with clangs and tones.

In Gamble and Calkins' (65) repetition of Lehmann's experiments with odors, they found a lesser percentage of recognitions without reproduced images and a greater percentage of recognitions which were attended by reproduced images than Lehmann had reported, but they conclude that reproduction is not necessary to the process of recognition. Their conclusion is based on the following facts: 1. correct reproductions are very often present in cases of non-recognition; 2. associations which were clear enough to be reproduced were not always present in cases where recognition occurred; 3. in cases where the order was noted, the attendant associations were usually reported as appearing after the recognition.

Severance and Washburn (69) instructed a number of observers to gaze steadily at various familiar words of six letters, and they found that the words soon began to lose all associative power, and hence became meaningless. A little later, they ceased to look familiar, and

finally, even the letters themselves were meaningless marks, conveying no sense of familiarity. The authors conclude that this loss of associative power and finally of all familiarity was due to the induction of abnormal sequences of the processes attended to.

Dearborn (17) employed ink blots as experimental material in an attempt to analyze the awareness of similarity and dissimilarity. One ink blot was selected as a norm, and placed in a frame convenient to the observer's eye. One hundred other blots were then presented simultaneously, the observer being asked to choose the ten which were most like, or most unlike, the norm. After the choice was completed, the observer described how the norm was apperceived and what was its most essential characteristic. The results show the significance of language in the observer's procedure,—criteria of likeness or difference being almost always sharply defined in words. Dearborn concludes that 'feeling' criteria (feelings of likeness or unlikeness) are more accurate than 'conceptual criteria,'—the observer whom he considered most successful having reported such 'feelings.' Dearborn refers feeling of familiarity to ease of following old cerebral paths, and feelings of unfamiliarity to a 'fumbling among the infinite possibilities of cortical routes.'

Using the method of partially learning nonsense syllables, Meumann (21) asked his observers to state, after each reading, whether or not the syllable just read was recognized as having been seen before. A new syllable was occasionally inserted in the list, as a control. Meumann finds that the impression of 'unknown' is a much more positive and definite experience than is the impression of 'known.' Five factors conduce to this: the sensation of hesitation, shock or weak fright; the interruption of ideation; the consciousness of void or emptiness; the characteristic feeling of unpleasantness; the non-appearance of the usual ideation. The 'feeling of familiarity,' on the other hand, he holds to be a product of: the facilitation of perception; accompanying feelings and organic sensations, carrying pleasantness and relaxed tension; less strained attention; the reproduction of ideas, often dim and scarcely discoverable.

Heine (66) undertook to determine whether subsequent disturbances have a retroactive effect upon recognition as they have been shown to have upon recall. Her method consisted in having her observers learn parallel series of nonsense syllables or four-place numbers, and then she introduced a disturbance by presenting, immediately after the learning, other materials (pictures, numbers, syllables) which were later to be described or reproduced. The recognition-time and the amount recognized both indicate that no retroactive effect took place. Similar experiments dealing with recall, instead of recognition, show the presence of a considerable retroactive disturbance. From these findings, Heine concludes that the factors involved in recall are fundamentally different from those involved in recognition, and that recognition is not dependent upon associative processes.

Meyer (68) set out to ascertain whether 'preparedness' favors the process of recognition as it does the process of reproduction. His method consisted in measuring the relative time and number of correct judgments of 'new' or 'old' when nonsense syllables which had previously been learned were presented in four different ways: an 'old' syllable preceded by its immediate antecedent in the learning series; an 'old' syllable presented alone; a 'new' syllable preceded by an accented syllable of a learned series; a 'new' syllable presented alone. His results show that those syllables which were preceded

by a 'preparing' syllable derive an advantage from that fact. The effect of this 'preparedness' for recognizing in consequence of the presenting of another associated syllable persists through a long expectation period even when other disconnected material intervenes. Meyer interprets the longer reaction-time and the lesser certainty in the case of unfamiliarity as an indication that the consciousness of 'unknown' is merely the absence of the quality of 'known'; and he quotes Meumann (21, p. 39) in support of this position. But instead of regarding unfamiliarity as a negative quality, Meumann holds that the quality of unknown possesses a much more definite character than the quality of known, and he even specifies its characteristics in detail (21, p. 38 ff.).

### III. EXPERIMENTAL

#### A. PRELIMINARY

1. *Repetition of Katzaroff's experiments.*—As a preliminary to our investigation, we undertook to repeat the work of Katzaroff. Our method was an exact duplicate of his; but we employed only thirty-six of his seventy-two drawings. Our experiment, therefore, while less extensive than his, was identical with it in the details of each sitting. Our observers included four men and four women, all of whom had had laboratory training in psychology; two were special students, and three were instructors in the department of experimental psychology.

Eighteen drawings were presented at each sitting, in groups of six, each drawing being exposed in a tachistoscope for six seconds, with an interval of four seconds between exposures. An interval of five minutes elapsed after the first and again after the second group of six. The observer was instructed to note each design carefully in order that he might be able to identify it later. He was told that after the series had been exposed, he would be shown these same drawings again, together with eighteen other very similar drawings, distributed irregularly among them. His task was to consist in determining whether the drawing presented in this subsequent series was identical with one seen in the first series, whether it was similar but not identical, or whether it was wholly new; and he was asked to distinguish four degrees of certainty of judgment,—absolutely sure, fairly sure, sure, and doubtful. Immediately after his judgment of each drawing of the recognition series, he dictated a detailed introspection to a stenographer, who was screened from view. The results of these experiments corroborated Katzaroff's findings in certain particulars, but they contradicted Katzaroff's findings in other particulars.

First, as to the rôle of the image in the act of recognizing: Katzaroff is convinced that imagery is not essential; he maintains that recognition has two distinct stages: first, the appearance of a feeling of familiarity; second, the appearance of images and memories. And he finds that the first of these is the initial process in every act of recognizing, while the imagery functions merely in a corroborative fashion. In an overwhelming majority of cases, our observers analyzed their recognitive consciousness into imagery and kinaesthesia, with occasional affective toning. However, cases in which a feeling of familiarity or of strangeness seemed immediate and unanalyzable were reported in a few instances by our observers. These unanalyzed

attitudes occurred most frequently in those cases where, as a variant of Katzaroff's procedure, we gave our observers a repetition of the same series at a later date. This finding suggested that the function of imagery may now have become less prominent because the material had become more familiar; and we made use of this suggestion in planning our next series of experiments.

Second, Katzaroff finds that the time required for making a judgment regarding designs which had already been presented but which had been forgotten, is greater than that required for similar designs which had not previously been seen; and he finds in this phenomenon a warrant for assuming that subconscious processes are in action here,—the material not being wholly strange in the former case. We have failed to confirm this difference in time; and we can see no justification for Katzaroff's appeal to that convenient hypothesis, the subconscious.

Third, in one other respect our results seem to warrant a rejection of Katzaroff's conclusions. He finds that recognition is essentially an affective process, and he believes that it is always enveloped in a consciousness of self. Our introspections fail to confirm this finding; in many instances, no trace of affection is to be found, and consciousness of self appears still more rarely.

In brief, we corroborated Katzaroff's findings in the following particulars: *a.* Imagery is lacking in many introspective descriptions of recognition; but we are not prepared to assert that imagery is never, or even that it is seldom essential to the cognitive consciousness. These preliminary experiments indicated that this consciousness has several, perhaps multitudinous stages, and that imagery plays a somewhat different rôle in each. *b.* The act of recognizing is frequently characterized by a conscious comparison of image and percept; this is especially true of the recognition of relatively novel objects. *c.* There is no correlation between subjective certainty and objective correctness.

We failed to corroborate Katzaroff's findings in the following particulars: *a.* The initial stage of every act of recognizing consists in the appearance of a 'feeling of familiarity'; and imagery serves only the secondary function of confirming or correcting this primary act of immediate recognition. *b.* Recognition is always an affective rather than an intellectual process, and it inevitably involves a consciousness of self.

*2. Preliminary experiments with photographs of persons and places, advertisements, block letters, different faces of types.*—For a second series of preliminary experiments we selected materials which, we hoped, our observers would find to possess widely different degrees of familiarity. We secured postcard views from the home cities, colleges, etc., of each of our eight observers, and among these familiar pictures we distributed views of Worcester, of Clark University, and others more or less familiar or wholly unknown; we also employed a series of Perry pictures of prominent men and women, and a group of more or less familiar advertisements. The eight observers who served on the previous problem served in these experiments also. Two methods of visual presentation were employed: First, the picture was either seen through an aperture giving a rapidly recurring series of instantaneous exposures in a rotating disc, or it was exposed by means of a mechanism which provided a continuous exposure but at a variable distance from the observer. The arrangement was

such that the picture could be presented at a distance where its details were imperceptible; then, while the picture was brought progressively nearer and nearer to the observer's eye, in continuous exposure, the details gradually emerged and became clear. The object in both these methods of presentation was to build up a process of recognition by gradual increments, so that the observer might be enabled to examine and give an account of its genesis and development.

Another series was designed to analyze the act of recognizing a tactual stimulus. Here we employed wooden blocks upon whose faces were cut letters of the alphabet, and digits, averaging eight centimeters in height; this series also included various objects such as a watch, a pipette, a small bronze medal, etc. The observer was blindfolded and the object to be recognized was placed in his hand. He immediately dictated a report of what went on in consciousness. If he failed to recognize the object he explored it still further, and then gave an account of the mental processes involved in his final attempt to identify it.

Data in all four of these series indicated that imagery played a very important rôle, and that it was supplemented by organic sensations and kinaesthesia. Imagery was the first and only content reported in the majority of cases. Affective toning was rarely reported, as also was consciousness of self. It was found that the observers usually adopted a procedure which consisted in reconstructing the situation in which the stimulus-object had formerly or usually been experienced in everyday life; this act of reconstructive recognition was usually characterized by a mass of imagery, sometimes detailed but often sketchy, in which the visual and the motor modalities predominated.

The results of these experiments indicate that imagery is of extremely variable significance in the act of recognizing. In certain instances, our observers reported a profusion of images which were vivid and rich in detail; in other instances their imagery was scant, indefinite, and schematic. Numerous introspections indicate that an inverse relationship obtains between abundance and richness of imagery on the one hand, and facility of recognizing on the other hand. In cases where the object or picture was relatively unfamiliar, and where the act of recognizing it was hesitant and difficult, the imagery was abundant and rich in detail. The discovery of this state of affairs suggested a method which promised to furnish a more detailed analysis of the recognitive consciousness, and a clearer insight into the component processes which contribute to the act of recognizing. And in all of our subsequent experiments we adopted a procedure which consisted in presenting materials which were unfamiliar at the outset, and in re-presenting them at intervals until they were recognized without difficulty. It was hoped that the introspective reports of these successive acts of recognizing stimuli, which must of necessity grow more and more familiar, would constitute essentially a series of cross-sections through the recognitive consciousness at successive points between its initial, hesitant stage, and its final, facile and mechanized stage.

In a final series of preliminary experiments, the materials chosen were alphabets printed from five different faces of type, each representing a characteristic variant from the common letter-form. These variant letter-forms were wholly unfamiliar to the observers at the outset; and the experiment consisted in having him describe the mental contents and the mental procedures employed in successive

acts of recognition at various stages throughout the process of learning. The lower-case alphabets were arranged in vertical columns, side by side, on a strip of white paper 6 cm. wide. This strip was placed on the drum of a Spindler and Hoyer exposure-apparatus, and the mechanism timed to furnish a three-seconds' exposure. The name of each face of type was plainly printed on the bevel of the exposure aperture, so that it would appear just below the column to which it belonged.

Six observers took part in this experiment. All had had training in introspection, and four of the six were specially trained students and investigators in experimental psychology. The experiments were begun April 3, 1912, and concluded June 6, 1912.

The observer was instructed to note the letters as they appeared (the row of five different "a's," then the "b's," etc.), in any fashion he chose, endeavoring to learn the characteristics of the different faces. After the exposure of the entire series, the observer was shown isolated letters from the list; he was asked to name the type to which each isolated letter belonged, and then to give a detailed description of the processes involved in his recognition, or attempted recognition. The observer was given his own time in the test series, the experimenter noting by means of a stop-watch the number of seconds required in each case. The sittings usually required from thirty minutes to an hour, though rarely the latter, so the element of fatigue was a negligible factor; the sittings were continued until the observer succeeded in recognizing immediately the 'face' to which each letter belonged.

The significant fact shown by these experiments is the marked decrease in the amount, variety, and clearness of imagery present in the recognitions as the letter-forms became more and more familiar to the observer. Many other points of interest appear in the results of these experiments,—some confirming beliefs widely held, some contradicting the results of other investigators in this field. Our data show that: 1. The time required for the act of recognizing decreases as the object becomes more familiar; 2. the degree of subjective certainty and the proportion of correct recognitions increase as the object becomes more familiar; 3. affective toning is wholly absent in the great majority of recognitive experiences; 4. no consciousness of self is present in the act of recognizing save in rare and exceptional instances; 5. the 'feeling of familiarity' rarely, if ever, appears as an unanalyzable and *sui generis* experience; 6. experiences of a motor sort become more and more dominant in proportion as the act of recognizing becomes more facile and more certain.

As the data accumulated, we discovered a serious defect in our method of presenting this series. By arranging all five alphabets side by side and exposing the letters in rows (all the "a's" in one row, all the "b's" in the next, etc.), we had put a premium on the spatial localization factor of memory; and our introspections showed that our observers tended to identify any given letter by referring it directly to the position in the row (first, second, third, fourth or fifth) which it had occupied during the exposure of the series. We believed this might account for the predominance of kinaesthesia in the recognitions of our observers (eye-movements, imaged or inner-vated, having been frequently reported), as well as for visual schemas which were common. We corrected this defect of method in the visual series of the final experiments.



## B. FINAL EXPERIMENTS

A. The preliminary experiments had shown that there is great diversity in the mental experiences which are known by the name of recognition. They had made it clear that it is not sufficient to mark off two great types of this process and to call *immediate* or *direct* that recognition which follows immediately upon perception,—where a given stimulus seems familiar the moment it is perceived, with apparently no intervening process whatever,—and *mediate* or *indirect*, that recognition in which associations appear in consciousness before or simultaneously with the consciousness of familiarity. Our introspective accounts had indicated not only that extreme cases of incipient and complete familiarity are characterized by more marked differences in the contents of consciousness than has usually been supposed, but also that there are several, perhaps many, intermediate stages between these two extremes.

We undertook to investigate these intermediate stages, as well as the two extremes; and in order to have a means of measuring and comparing them in the course of their development we devised a genetic method. We chose materials which were novel and taught them to our observers, taking an introspective cross-section of the recognitive consciousness at numerous levels between the initial and the mechanized stages.<sup>3</sup> The problem was attacked in four sense departments,—visual, auditory, tactual, and olfactory.

*A. Method and Material Employed in Experiments in the Recognition of Visual Stimuli.*

The materials chosen for this experiment were five different faces of type,—Della Robbia, Bulfinch, American Typewriter, Clearface, and DeVinne. The capital letters of a given face were arranged vertically, in alphabetical order, on a band of paper designed for exposure on the drum of a Spindler and Hoyer exposure apparatus,—a device which gives a series of exposures, each of which is made during a pause in the movement of the revolving drum. The printed name of the type was placed just above the exposure apparatus. The exposure-time allowed to each letter was four-tenths of a second. The observer was seated before the apparatus, and was told that he was to observe the letters with the purpose of being able, eventually, to recognize any one of them as belonging to a particular face of type. He was told, further, that he was not to feel constrained to learn these types at one, two or half a dozen sittings; he was made to understand that the experimenter was not interested in the quantitative results, but rather in the introspective accounts of what goes on in mind when a given visual stimulus is recognized as familiar. The name of the type was pronounced to the observer, then the 'ready' signal given, the apparatus set in motion, and the shutter

<sup>3</sup> We shall use the term 'initial recognitions' to designate the first or earliest of our observers' identifications of the material learned,—those recognitions in which there was usually only a slight degree of familiarity. The name 'final recognitions' will be given to those later experiences in which the observers' familiarity with the stimuli was complete, but in which it was still a conscious content. This study is not concerned with mechanized experiences which are practically reflex reactions to stimuli, and in which no consciousness of familiarity is present.

opened. The entire alphabet was displayed from beginning to end. At the close of the exposure, the observer gave an introspective account of his procedure, which was taken down by a stenographer. In this manner, all five of the faces of type were displayed in turn, but in different order, at each sitting.

At the beginning of the second and of each subsequent sitting, tests were made of the observer's familiarity with the types in the following manner. Single letters of the various types, printed on cards, were exposed in exactly the same position as that occupied by the letters in the learning series. The observer was asked to report, as soon as he could, whether or not he recognized the face to which the letter belonged, and to give an account of everything which was present to consciousness from his perception of the stimulus until his recognition was reported, the time required for the recognition being recorded by means of a stop-watch. The observations were made once each week, from March 3, 1913, to May 12, 1913.

Four observers served in this experiment, all students or instructors in the department of psychology in the University. They were: *F.*, instructor in the department of experimental psychology; *Fn.*, laboratory assistant and fourth year graduate student in experimental psychology; *Fs.*, fourth year graduate student in experimental psychology; *W.*, first year graduate student in experimental psychology. The observers did not discuss the problem either with each other, or with the experimenter.

#### *B. Method and Materials Employed in Experiments in the Recognition of Auditory Stimuli*

The material chosen for this part of the experiment comprised various airs and themes from operas, symphonies, songs, etc. Care was taken to select for each observer only such compositions as were entirely unfamiliar to him when the experiment began. The following is a list of the selections employed, either as a whole or in part: *Tschaikowsky*, Danse Trepak, Overture 1812, Song without Words, Sleeping Beauty Ballet, Fifth Symphony; *Reissiger*, Yelva Overture; *Humperdinck*, Hansel and Gretel; *Verdi*, Rigoletto, Il Trovatore and La Traviata; *Leybach*, Fifth Nocturne; *Tobani*, Songe d'Automne, Hungarian Fantasia; *Goldmark*, Brautlied. The selections chosen were all orchestral, in order that, so far as possible, the recognitions should have to do with the air alone.

The music was played on a Victor Talking Machine, placed in a box in a room adjoining the experimenting room. The sound was conveyed to the observers' ears through the wall by means of a rubber tube which terminated in a pair of stethoscopic ear-tubes. A stop-cock in the tube, between the phonograph and the wall, enabled the experimenter to control the part and amount of the selection which was presented to the observer.

The observer sat in a comfortable chair, having on its right side a broad arm to which was screwed a reaction-key communicating with another key placed on the phonograph box. He was instructed to give one tap on the reaction-key when the music became familiar, and two or more taps when he could identify the selection. When this latter signal was given, the music was instantly shut off; and the experimenter recorded the observer's introspective report of his mental processes from his perception of the ready signal to the appearance of his recognition of the selection.

At the first sitting, from three to seven airs were presented to the observer, the name of each being given him in both visual and auditory fashion. At the close of the selection, the observer gave a detailed account of his procedure throughout. This was done after each act of learning as well as after each test of recognition. At later sittings, besides a new air or two, the selections already played to the observer were given him again and if he failed to recognize them, the experimenter named them and played them through once more. The time required for each recognition was recorded by means of a stop-watch. Occasionally an entirely new air was played and this check brought out some very interesting experiences of non-recognition which will be discussed later.

This series of experiments was begun in November, 1912, and continued with each observer until the recognition of most of the selections had become immediate. The last sitting took place February 19, 1913. Six observers took part in these experiments,—all students or instructors in the psychological department of the University. They were *F.*, *Fs.*, *W.*, and *Fn.*, who assisted in the visual series already described, and *V.* and *B.*, third year graduate students in psychology.

#### *C. Method and Material Employed in Experiments in the Recognition of Tactual Stimuli*

The method employed in this series of experiments was essentially the same as that used in the visual and auditory series. Our observers were taught to recognize tactual stimuli which had been wholly novel and unfamiliar at the outset; and introspective cross-sections were taken throughout the course of the mechanization of their acts of recognizing these stimuli.

The materials employed were the letters of the alphabet for the blind (New York Point). In the learning series, the blindfolded observer sat in the ordinary writing position, with his right arm resting upon a frame within which the letters were presented. He was instructed to explore with his finger-tip each tactual stimulus which would be presented, and to learn to recognize these stimuli when they should subsequently be given to him. A sheet containing a single letter was then placed in the frame; and at a given signal, the observer began his exploration, the experimenter pronouncing the name of the letter. Thirty seconds were allowed for each exploration in the learning series; and the observer gave an introspective account of his procedure at the expiration of that time. Not more than five new letters were presented at a single sitting.

In the recognition series, the observer was asked to explore the stimulus and to report whether or not it was familiar; in the case of a familiar stimulus, he was instructed to recall the name if possible. The time required for the act of recognition was noted in each case, and a stenographer recorded the observer's introspective report of his procedure. When he failed to recognize a letter, or when he made a false recognition, he was asked to explore the same stimulus again and the correct name was supplied by the experimenter. This procedure was continued until the recognitions were made without hesitation. Five of our observers: *F.*, *Fn.*, *Fs.*, *V.*, and *W.* took part in this series of experiments. The observations began October 31, 1912, and closed February 24, 1913.

*D. Method and Materials Employed in Experiments in the Recognition of Olfactory Stimuli*

In a fourth series of experiments, observers were taught to recognize a number of different odors, including: (1) Novel perfumes,—*Quelques Fleurs*, *Acme Violet*, *Coast Violet*, *Trèfle*, *Halcyon Rose*, *Sous Bois*, *Bouquet Janice*, *Bruyère Rêve*, and *Flowers of Savoy*; (2) a series of alcohols,—*Ethyl*, *Methyl*, *Amyl* and *Butyl*; several coal tar products,—*Cresol*, *Phenol*. Other odors were often used as controls.

These substances were kept in bottles of uniform size, and they were handed to the blindfolded observer, who removed the stopper and sniffed the odor in the manner that seemed most natural and convenient to him. In the learning series, which never exceeded eight in number, he was told the name of the odor, and allowed to investigate the smell quality as long as he wished. As he dictated an introspective account after each experience, the olfactory membrane had ample time to recover between stimulations. The interim always exceeded the time found by Zwaardemaker<sup>4</sup> to be necessary for recovery from complete fatigue of the membrane,—a condition which occurred in one case only during the entire experiment.

In the test series, the observer was given various of the bottles, one at time, and asked to identify each odor if he could. He was allowed to signify his recognition by handing back the bottle, giving the odor's name, or in any way that he chose. In each experience, the time consumed from the presentation of the stimulus (the actual sniffing of the odor) to the observer's reaction signal was recorded and his introspection was taken down by a stenographer.

Seven observers took part in this experiment: *F.*, *Fs.*, *W.*, *V.*, *S.*, *O.*, and *Bd.* The last three served only in the olfactory series. *O.* and *S.* were graduate students in psychology, *Bd.* is Professor of Psychology and head of the Department of Experimental Psychology.

## 2. RESULTS

*A. Introspections.*—In this section we present typical introspections obtained from each observer in each series of these experiments.

### *Observer F.*

#### *Introspections on the Recognizing of Visual Stimuli*

1.<sup>5</sup> April 2, 1913. Clearface 'A,' 10 sec. (8th. presentation; first correct recognition.) "As soon as I saw the letter I was conscious of a vocal-motor image 'big, thick,—what is that thing?' There was a decided bodily strain, rather widely diffused; I repeated 'big, thick,' over and over, in vocal-motor imagery, until suddenly 'big, thick Clearface' rushed into consciousness in vocal-motor and auditory terms, the accent on the 'Clearface' expressing my annoyance at being so slow. I tapped the key at that point."

2. April 16, 1913. Clearface 'A,' 7 sec. (9th. presentation.) "On perceiving the stimulus I found myself repeating 'big, thick' in vocal-motor auditory imagery. It was very familiar. I went through the

<sup>4</sup> H. Zwaardemaker, *Die Physiologie des Geruchs*. Leipzig, 1895.

<sup>5</sup> The serial number at the beginning of the introspection, to the left of the date, is a purely arbitrary number which is inserted here for convenience in subsequent reference.

list of types in this manner, in vocal-motor auditory imagery: 'big, thick Della Robbia; big, thick. De Vinne; big, thick American Type-writer; big, thick Clearface.' Then I repeated this last phrase quickly and with assurance. There was increasing tension throughout these processes, with relaxation of strain at the last, as soon as I tapped the key."

3. April 24, 1913. Clearface 'A,' 1 sec. (10th, presentation.) "As I became aware of the visual stimulus, I had, in vocal-motor auditory imagery, 'big, thick Clearface,' with relaxation of tension and absolute surety."

4. April 30, 1913. Clearface 'A,' 6 sec. (11th. and last presentation.) "On perceiving the letter I was conscious of immediately tapping and saying 'Clearface.'"

### *The Recognizing of Auditory Stimuli*

5. Dec. 10, 1912. 'Overture 1812,' 10 sec. (1st. recognition.) "This air seemed slightly familiar almost from the first. My consciousness was entirely filled with vocal-motor and auditory images of the air, which anticipated the tones that were being played. There was a slight tension throughout my whole body. When the 'Russian National Hymn' came, the words 'Hail, Pennsylvania' appeared in vocal-motor imagery, with some innervation. Then vocal-motor '1812' followed in consciousness, and I promptly reacted on the key. Tension relaxed when the '1812' image appeared."

6. Jan. 16, 1913. 'Overture 1812,' 3.6 sec. (2nd recognition.) "The first thing of which I was conscious after the music began was the vocal-motor imagery 'I know that.' There was tension in forehead and eyes, but no images of humming. Then '1812' suddenly appeared in consciousness in vocal-motor imagery; tension relaxed and I found myself reacting."

7. Jan. 29, 1913. 'Overture 1812,' 1.4 sec. (3rd. recognition.) "Vocal-motor and auditory imagery of the word 'Hail,' sung in my own voice, and prompt reaction on the key, with relaxation following. The name of the selection came immediately afterwards, but was not a part of my consciousness of familiarity."

8. Jan. 29, 1913.<sup>6</sup> 'Il Trovatore' (Verdi), 3.8 sec. (5th. recognition.) "Vocal-motor imagery of humming the air, anticipating the music after the second note. Then in rapid succession came the vocal-motor images 'Verdi group; Trovatore.' Slight muscular tension at first, diffused over the body, which relaxed at the word 'Trovatore.'"

9. Jan. 29, 1913. 'Brautlied' (Goldmark), 2 sec. (7th. recognition.) "Keen attention to auditory stimulus at first with nothing in consciousness but the auditory perception. With the fourth note,—that sliding one,—distinct vocal-motor imagery of singing that sliding note myself, followed by vocal-motor 'Goldmark's Bridal March,' and instant relief."

### *The Recognizing of Tactual Stimuli*

10. Nov. 27, 1912. 'a' (Two dots in a horizontal line.) 7 sec. (4th. presentation; 1st. recognition.) "When I got this stimulus in

<sup>6</sup> In addition to the progressive series of introspections, we have included here certain other introspections which illustrate points to be discussed later.

tactual terms I was aware of an auditory and vocal-motor image of 'a,' with a questioning accent. I had no high degree of certainty, however; and I inhibited my reaction and explored the stimulus further. A visual image of three dots arranged in a horizontal line came to consciousness, and with it a vocal-motor auditory 'f.' Then I was aware of a series of auditory vocal-motor images somewhat as follows: "'a' used oftener, simple, 'a,'" meaning that 'a' occurs more frequently than 'f' in actual use and therefore the simpler form of two dots would be employed to represent it. This, then, is 'a.' There was a diffuse bodily strain throughout until this last process, with relaxation at the end."

11. Dec. 12, 1912. 'a,' 2 sec. (6th. presentation.<sup>7</sup> "As soon as I got the tactual perception I had a vocal-motor auditory image of 'two dots.' Then I was aware of a tactual image of two dots, and a kinaesthetic image of reaching toward the right; 'a' followed immediately in vocal-motor auditory imagery."

12. Jan. 20, 1913. 'a,' 1.4 sec. (8th. presentation.) "I had a kinaesthetic and tactual impression of two dots in a horizontal position; and immediately afterwards a vocal-motor auditory image "two dots, 'a.'"

13. Feb. 6, 1913. 'a,' .8 sec. (9th. presentation.) "A vocal-motor auditory image of 'a' came to consciousness immediately with my awareness of the number of dots in tactual terms, and of their horizontal position in kinaesthetic terms. Then I was aware of turning from the problem with instant relief."

### *The Recognizing of Olfactory Stimuli*

14. Jan. 24, 1914. Ethyl Alcohol, 20 sec. (3rd, presentation; 1st. recognition.) "I sniffed it; and 'alcohol' came to consciousness in vocal-motor imagery. I was distinctly aware of the cooling sensation in my nose. It seemed familiar but no name came to me, and I was aware of unpleasantness, and the words Which one? Don't know. I'll never get them.' I kept on smelling, but the unpleasantness increased, and a general feeling of despair took hold of me,—a sinking organic sensation, which seemed somehow to include my whole body."

15. Jan. 31, 1914. Ethyl Alcohol, 17 sec. (4th. presentation.) "On perceiving the coolness which my first sniff of this produced in my nose, I said, 'Alcohol.' This was in vocal-motor imagery, with a rising accent. Then I smiled and said, 'Yes, alcohol. Let's see,—Ethyl or Methyl or Butyl?' Then I was aware of getting a drop of it on my nose and immediately had, in vocal-motor imagery, 'Now, I'll cough!' There was then a perceptible moment of waiting for that cough, and when it failed to appear, the Butyl possibility slipped out of consciousness altogether, and I said 'Methyl or Ethyl, not sure which.'"

16. Feb. 14, 1914. Ethyl Alcohol, 14 sec. (5th. presentation.) "I approached it with a definite perfume-*Einstellung*. I was saying to myself, 'They alternate, perfume,—alcohol—benzine group—perfume.' I took several whiffs before I could decide whether it was alcohol or perfume. This was present in terms of vocal-motor imagery of

<sup>7</sup> It sometimes happened that successive introspections in a series were practically identical with one another. These duplicate introspections have not been included here,—the omission being indicated by a discontinuity in the serial numbers of (presentations or) recognitions.

this sort: 'Not perfume, alcohol; no, perfume; no, alcohol,' etc.; finally, 'mild alcohol.' Then I was aware of the coolness in my nose and attention was focused on this. The odor began to seem definitely familiar and I said 'cool in nose, mild alcohol, Ethyl.' My signal-tap ushered in distinct relaxation."

17. Feb. 21, 1914. Ethyl Alcohol. 9 sec. (6th. presentation.) "I was aware of the quality of the odor and then of vocal-motor 'alcohol? Not Butyl or Amyl; what—Ethyl, Methyl? Oh! cool in nose, Ethyl.' Then I turned to you, pleased with my success."

18. March 28, 1914. Ethyl Alcohol, 1 sec. (7th. presentation.) "Immediate vocal-motor 'alcohol—cool, Ethyl.' A high degree of certainty as I tapped."

### *Observer Fn.*

#### *The Recognizing of Visual Stimuli*

19. April 21, 1913. Bulfinch 'E,' 8 sec. (5th. presentation; first correct recognition.) "The perception came in slowly and aroused a memory experience of myself going through this experiment at a former sitting. There was visual imagery of the room and the apparatus, and kinaesthetic and organic images of myself experiencing it all. Then followed imagery of a sitting previous to that in which I had noted this letter in the Bulfinch series. Two places in my visual schema claimed my attention here, and although there had been consciousness of familiarity throughout, it gradually increased in intensity until the names of those two places came in vocal-motor imagery: 'old Clearface,—Bulfinch.' The latter name was stressed and with its appearance the Bulfinch place in my schema<sup>8</sup> became focal in consciousness."

20. April 28, 1913. Bulfinch 'E,' 1 sec. (6th. presentation.) "My perception of that letter thrilled me all over,—I was especially conscious of sensations of thrills in my chest. I localized this type in my schema, and the schema in my experience of last week; it was sufficient to make me absolutely certain. I said 'all right'; and it was not until after that that the name 'Bulfinch' appeared in vocal-motor imagery."

21. May 5, 1913. Bulfinch 'E,' .4 sec. (7th. presentation.) "The perception itself bore an intensive consciousness of familiarity, which seems to consist in its ease and readiness; the experience culminated in the instantaneous relaxation of the initial tension, as the Bulfinch part of my schema flooded into consciousness."

#### *The Recognizing of Auditory Stimuli*

22. Jan. 11, 1913. 'Brautlied' (Goldmark), 10 sec. (5th. presentation; first recognition.) "Four or five notes had gone before I realized that I was familiar with the air. My familiarity seemed to consist in the facility of my perception of the tones. I found myself turning back to repeat the first part in auditory imagery. My visual schema for the Tschaikowsky group came to consciousness vaguely,—indeed, it was so dim that I was not conscious that it was my Tschaikowsky schema until afterwards. It was merely a dim visual image of five gray blotches, and this air did not seem to fit into it,—no definite associations with this air occurred and the schema dis-

<sup>8</sup> For description of this schema, see p. 351.

appeared from consciousness. Then I visualized a long note, on a music score, with four short notes straggling down from it, and then four others ascending the scale again." (This was *Fm.*'s schema for the 'Yelva Overture.') "My auditory imagery would not fit into that schema, either, so that possibility was discarded. Suddenly, with no discoverable antecedent, 'Natoma' occurred to me in vocal-motor and auditory imagery, and I was conscious of innervation in my reacting arm; but the reaction was inhibited by a new process,—I actually turned around and looked over across the table where, for some reason, I have always located 'Brautlied.' My whole attitude of body and mind was a questioning one. Then I turned back satisfied, tension relaxed, and I found my hand pressing the key. I was not aware of the name 'Brautlied' until afterward."

23. Jan. 27, 1913. 'Brautlied,' 5 sec. (2nd. recognition.) "Intense excitement. Familiarity just stirred my whole body. The whole situation in which I last heard this selection came up in visual imagery. Then 'Tschaikowsky' appeared in auditory imagery, but my attention turned from this immediately; and 'Brautlied' in auditory imagery, together with a humming of the air, filled my consciousness and I reacted."

24. March 3, 1913. 'Brautlied,' 1 sec. (3rd. recognition.) "The very first note had a perfectly definite familiarity,—simply thrilled me all over. That familiarity seems to be the sum total of my bodily reactions to the music. The name 'Brautlied' did not occur until a moment later, when it appeared in vocal-motor and auditory imagery."

#### *The Recognizing of Tactual Stimuli*

25. Nov. 23, 1912. 'e' (A single dot.) 16 sec. (2nd. presentation; 1st. recognition.) "As soon as I got a tactual impression of a single dot, I recalled the past situation here, in an endeavor to remember if any letter which had been presented had consisted of but a single dot. There was no familiarity in the actual experience, and I felt sure that the experimenter would never have given me so simple a figure. I attended keenly to the stimulus, and to both the visual and tactual imagery of other letters which had formerly been presented; 'g,' 'f' and 'h' stood out clearly. Then 'e' came to consciousness in auditory imagery of the experimenter's voice. Still no familiarity appeared. Then I had a tactual image of one dot, together with a visual image of a letter 'e'; I was not sure whether these were images of memory or of imagination. Still my image of the letter 'e' took its place in a visual schema which was then referred to the first day's experience and I reacted."

26. Dec. 14, 1912. 'e,' 5 sec. (4th. presentation.) "The tactual sensations called up a memory of the whole situation in which this stimulus was last presented,—not in visual imagery alone. The experience is rather that of living over again the former experience with its tactual imagery of this single dot, and auditory imagery of my own voice calling it 'e.'"

27. Dec. 21, 1912. 'e,' 2 sec. (5th. presentation.) "I perceived a single dot immediately, but my attention was directed to an attempt to find more than one. I was conscious of exploring rapidly all around this dot several times. Then the 'e'-ness of that single dot became focal and had a sort of familiarity which seems to consist simply in my tendency to attend to it, though my finger was still



exploring for others. About this time, I called this stimulus 'e,' pronouncing the name aloud."

28. Feb. 15, 1913. 'e,' .4-sec. (8th. presentation.) "I had no more than touched the stimulus when a perception of the singleness of the dot came to consciousness. There was a visual image of the one dot, and just an immediate flash of familiarity which seemed to consist in the way the sensation attacked me,—the rapidity with which it came to consciousness, and the permanence of its quality. I was conscious of a motor response to it,—a sort of diffuse bodily thrill, faint, yet distinct. There is always something of this bodily thrill in my recognitions."

### *Observer Fs.*

#### *The Recognizing of Visual Stimuli*

29. March 11, 1913. Clearface 'E,' 2.6 sec. (2nd. presentation; 1st. recognition.) "I examined this letter very carefully, noting the serifs and the hair lines; my attention seeming to turn naturally from one feature to another. My visual schema came into consciousness; and this letter located itself toward the last of the five blotches of the schema,<sup>9</sup> one of which seemed to be more clearly defined. I was conscious of a mental effort to name all the blotches. The second from the top I called 'Bulfinch,' in vocal-motor imagery; then I named the one above it 'American Typewriter.' There was tension then, and a period of suspended action before I corrected this by calling the first 'Della Robbia' and the middle one 'American Typewriter.' Then my attention turned to the last two, vacillating between them for a time; but finally I paused for a perceptible interval on the very last. At the same time the name 'Clearface' came to consciousness in vocal-motor imagery and I reacted. There is no high degree of surety even now."

30. March 17, 1913. Clearface 'E,' 1.6 sec. (2nd. recognition.) "The first thing I noted was the general size and heaviness of the letter, and the difference in the widths of its lines. All of this was present in my visual perception of it. Then I had, almost immediately, a visual image of 'Clearface,' the word beginning with this particular 'C.' This was localized at the end of my visual schema, the rest of it being barely present to consciousness. I was not aware of any affective toning. I immediately tapped on getting my visual cue."

31. April 3, 1913. Clearface 'E,' 1 sec. (3rd. recognition.) "I perceived that the letter was large and had clearly marked differences in width of line. There was an immediate tendency to set my mouth for saying 'Clear—,' chiefly imaginal, I think. My attention was increasing all the while and my uncertainty decreasing. Finally, I said 'Clearface' and attention shifted immediately. There was scarcely any awareness of familiarity this time."

32. April 17, 1913. Clearface 'E,' .4 sec. (4th. recognition.) (The observer had been instructed, this time, not to react, nor to try to recognize, but simply to observe the stimulus-letter, and later to tell her experience.) "I was aware of an attention to the letter, of noting the widths of its lines together with its size and general boldness. The word 'Clearface' rushed into mind instantly, the syllable 'Clear' in vocal-motor, and 'face' in auditory imagery."

<sup>9</sup> For a description of this schema, see pp. 354 f.

33. May 12, 1913. American Typewriter 'O,' .6 sec. (12th. recognition.) "I was immediately aware of the size and narrowness of the letter. My recognition of it was instantaneous and consisted in a tendency to react, *i.e.*, an awareness of tension in my right hand and about the muscles of my chest, as though I were holding my breath. The name appeared an instant later, in vocal-motor imagery, in very much abbreviated form: 'Am. Tyr.'"

*The Recognizing of Auditory Stimuli*

34. Nov. 21, 1912. 'Natoma' (Victor Herbert), 10 sec. (2nd. presentation; 1st. recognition.) "With the first phrase of the music, I experienced a sudden kinaesthetic relaxation and a forward movement as though to get nearer the source of the sound. 'Dream of Autumn' came to consciousness in auditory vocal-motor imagery; then 'Tales from Hoffmann.' I had a strong tendency to react; I was aware of tension in my right arm. My foot was tapping, too. Suddenly that measure which contains the drum beats claimed my attention, and 'Natoma' rushed into consciousness in vocal-motor imagery. That process was accompanied by a forward movement of my whole body,—a thrill all over,—and then I reacted."

35. Dec. 2, 1912. 'Natoma,' 8 sec. (3rd. recognition.) "There was a slight consciousness of familiarity from the first. I paid close attention to the music and followed the rhythm slightly with bodily movements. I thought it was from some opera,—the word 'opera' being present in auditory vocal-motor imagery and after it 'Carmen.' (That merely by way of association.) This possibility was immediately discarded,—just slipped out of mind. Then I was aware of tension,—of a slight forward movement, and 'Natoma,—why, of course, Natoma,' came to consciousness in vocal-motor auditory imagery."

36. Jan. 16, 1913. 'Natoma,' 5 sec. (5th. recognition.) "At first I was keenly aware of the two chords and their minor quality. The air had a definite familiarity. There was slight tension, rather diffuse, and 'Natoma' appeared in vocal-motor imagery. With that I had a tendency to react,—this in terms of a vague awareness of kinaesthesia in my reacting arm. The reaction was inhibited by a recollection of having confused 'Natoma' with 'Dream of Autumn' before,—this being present to consciousness in terms of a visual image of two names printed one after the other, and some visual imagery of this room and of the situation at a former sitting. My certainty that this was 'Natoma' increased. The air went on and finally the 'Dream of Autumn' faded out of my imagery and I reacted."

37. Jan. 28, 1913. 'Natoma,' 3 sec. (7th. recognition.) "I fell into the rhythm immediately; felt my head and shoulders swaying with it, and started to react. It was perfectly familiar. Then the word 'Natoma' occurred in vocal-motor imagery and I gave the signal."

38. Jan. 16, 1913. 'Danse Trepak,' 7 sec. (7th. presentation; fourth recognition.) "With the first note there was perfect awareness of familiarity with the music. Could not react rapidly enough. The consciousness of familiarity was made up of two components: a rapid impulse toward the reaction-key, and a visual image which seems to be a product of that old image of the disc containing five selections, but this time it appeared as five very gray blotches which might be manuscript music. The upper blotches seemed clearer, and attention

was directed toward them. I wanted to give the signal because I was well aware that I knew the name of that air, but the reaction was inhibited. This is my interpretation of my awareness of strain in my poised, reacting hand."

39. Jan. 28, 1913. 'Danse Trepak,' 1.4 sec. (9th. presentation.) "On getting the first auditory sensation I immediately started following it out in vocal-motor and auditory imagery of humming and gave two taps, automatically. The absolute automatic perfection with which I fell into that music just seemed to be accompanied by the two taps. *Afterwards* I started to recall the name, getting a visual image of a phonograph disc, larger than the usual full-sized discs, and having five distinct divisions in its threads, marked by deeper grooves.<sup>10</sup> My attention was directed to the first of these divisions, coincident with vocal-motor image of 'what' which meant: 'What was the name of that?'"

40. Dec. 2, 1912. 'Traviata' (1st. presentation.) "There was no familiarity throughout the whole experience. The rapidity of the music first brought up a visual image of the disc with the five airs on it. This was non-focal, attention all the way through being on the music. The first tempo was very pleasing. I noticed that the trills were being played differently. The word 'new' in vocal-motor and auditory imagery was present with a decided inflection. There was intense kinaesthesia in the feet and hands, and I even went so far as to press the key automatically. My whole body seemed to swing; but no name occurred, nor was I aware of any search for one."

#### *The Recognizing of Tactual Stimuli*

41. Jan. 27, 1913. 'f' (The 'f' is represented by three dots in a horizontal line.) 10 sec. (5th. presentation; 1st. recognition.) "Just as soon as I became aware that the dots of the stimulus were in a straight line, the word 'what' came to consciousness, in a setting of the vocal-motor apparatus for saying 'what?' With this I had visual images of several different arrangements of dots which seemed to represent experiences similar to this in the past. A visual image of the letter 'a' appeared also, and all of this went to make up my attitude of 'What letter is this?' rather than 'Have I seen it before?' This attitude seemed to initiate my consciousness of familiarity with the stimulus. I then re-explored the group of dots to find the exact number of dots; and when I perceived that there were three, this attitude became still more intensive. I remembered that I had had this group of dots before, the remembrance consisting in a visual image of a card having three dots on it, together with bits of imagery of this room and the general situation. My 'what-is-it?' attitude persists, but I can not name this letter." (At this point in the introspection, the name of the letter was told to the observer.) "Now I remember; and the recall came in motor fashion. When I was learn-

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<sup>10</sup> The reference to a visual image is an experience which *Fs.* frequently described in connection with the five different Tschaikowsky selections, all of which were on the same disc. Danse Trepak was the first in order. In the initial learning the entire record was played through, a list of the names of the airs having been given the observer. This image ran a most interesting course in consciousness, gradually losing its details until it finally became little more than a visual schema.

ing this letter I associated it with the deaf and dumb alphabet in which the 'f' is made by passing the right palm quickly in a straight line across the up-turned left hand. That gesture came up in a flash of motor imagery when you mentioned the name."

42. Feb. 17, 1913. 'f,' 3 sec. (6th. presentation.) "That is 'f.' As soon as I got the tactual and kinaesthetic perception of the dots, arranged in a straight line, I had a kinaesthetic image of that manual motor movement which represents 'f' in the deaf and dumb alphabet. I also visualized my hands making the movement, then the 'f' came as a vocal-motor innervation. The whole experience seemed very familiar."

43. Feb. 24, 1913. 'f,' .8 sec. (7th. presentation.) "I got a tactual perception of a straight row of dots, a motor image of the movement for the 'f' of the deaf and dumb alphabet; and I named the letter immediately, with full certainty."

#### *The Recognizing of Olfactory Stimuli*

44. Jan. 31, 1914. Trèfle, 4 sec. (2nd. presentation; 1st. recognition.) "Trèfle. First I was aware of the fragrance of it in terms of attention to the odor itself. I was aware that it was a stronger and more intensive fragrance than the first I had had. That was an interpretation of a state of attention to the intensity of this odor itself, other things not being in consciousness. I do not think that I was actually aware of what I was comparing it with. Then there was the briefest possible visual image of a bottle inclosed in a basket casing which I now know was the same one I had associated with the word 'Oriental' once before. That seemed to function as a consciousness that it was not one of my distinct flower odors,—a consciousness, largely of the fact that more flower odors occurred to me as a possibility. As the word 'Trèfle' appeared it was unhesitatingly spoken. It appeared in auditory and vocal-motor verbal imagery; I was neither sure nor unsure; it was not a genuine recognition. I am not very certain. It seems that perhaps Trèfle was the first of this series that occurred and I spoke without thinking, almost. What I mean is that I can not characterize my certainty at the time because there was neither uncertainty nor certainty."

45. Feb. 7, 1914. Trèfle, 19 sec. (3rd. presentation.) "I think I said that was something that contained the least suggestion of cologne about it before. Marked recognition of it as a perfume. I was aware of a little tension, very slight, located largely in the throat, above the vocal organs. First I got the whiff and then this tension disappeared and there was a tendency to take the bottle away from my nose. Almost immediately started to say the word 'what' with renewed and much more keen attention to the odor than ever before. Took a deep whiff of it. No name whatever occurred and I was aware of slight unpleasantness and tension. Then I held the bottle down at my knee for a moment, all this time trying to think what it might be. Aware of this suspension in consciousness and this tension. Did not make any effort to reconstruct the past series. Then smelled the thing again, attending now to one thing and now to another. Tried to get an identifying quality but without success. Finally I thought I detected a slight suggestion of freshness that either I told you about or registered mentally as a criterion for one of these odors. That came simply as a visual image of a cologne bottle."

46. Feb. 14, 1914. Trèfle, 10 sec. (4th. presentation.) "Very uncertain, but it might be one of those violets. First of all I was aware that it was a fragrant odor and there was a slight pleasantness connected with that. That was purely in the nature of the smell. I found myself taking rather a deep whiff without any special effort to determine what it was; it was rather a mechanical response to the general situation of having the thing in my hand. Toward the close of that whiff, I became aware of the problem in hand. That was in a marked concentrating of my attention much more closely upon the smell, in a cessation of pleasantness, and I think a general but very slight kinaesthetic pulling up to the task, or readjustment. I should not say general,—it was localized in my chest and trunk. Then my attention singled out a very slight something which increases the violet quality. That is simply in terms of attention to an aspect of the smell. The word 'violet' occurred without any particular inflection,—either questioning or decision. There was another alternative. Almost immediately I became aware of a vague spatial schema,—I seemed to be attending to a grayish visual splotch, to the left of which was another very much less distinct splotch. That state of affairs was a consciousness that the odor was not very well identified as violet and that it might belong to that group of non-flower odors. I never reached any great degree of certainty."

47. Feb. 21, 1914. Trèfle, 12 sec. (5th. presentation.) "There was a short period, at first, during which I was adjusting myself to the fact that the odor was a perfume. I simply did not try to select out any characteristic quality at the outset. I soon began to do this and then had a peculiar schematic consciousness of discarding all the flower odors. This came in visual imagery of a table, with about twenty-five bottles on it. (I did not recognize it then, but now I know it was a Wellesley scene.) Then these receded as though pushed away by an arm,—which, however, I did not see. All the time my attention was partly occupied, too, by a non-flower-like quality in the fragrance. I then took a deep whiff, and the word Trèfle appeared. Still I did not have a tendency to turn away from the problem, as happens when the name comes with great subjective surety. I smelled it once more, and was aware of a certain delicacy in the fragrance. Then I said, 'That's Trèfle,' exhaled my breath, and relaxed. I was sure I was right."

48. March 14, 1914. Trèfle, 2.2 sec. (6th. presentation.) "Is that Trèfle? Took a fairly deep whiff of that,—the rather strong non-flower quality of it stood out. 'Quelques Fleurs' occurred for an instant in clearly auditory terms,—possibly visual, too. Immediately I attended away from the Quelques Fleurs. Then the word 'Trèfle' started to say itself. Barest sort of set of the mouth for the 'T' sound. Then a little catch of the breath and I asked very suddenly, 'Is it Trèfle?' There seemed to be no affective toning in that experience. It was more a sort of suspense, waiting for the answer. Of course the awareness of the perfume was present all through this."

49. April 4, 1914. Trèfle, .8 sec. (7th. presentation.) "That is Trèfle. That was an immediate recognition. The odor was mild at first. While I attended to it carefully I did not get a very satisfactory whiff of it until rather late in the first whiff. Just as soon as it stood out my attention focused on a peculiar quality of mildness,—lack of that overpowering sweet quality. This had a milder and different quality. My attention pounced on that. It stood out very suddenly and I said afterwards 'That is Trèfle.' The words

seemed to come spontaneously and I said them with an inflection of certainty."

*Observer W.*

*The Recognizing of Visual Stimuli*

50. March 7, 1913. Della Robbia 'N,' 10 sec. (2nd. presentation; first recognition.) "The moment I perceived it, there was a slight familiarity; a whole mass of associations came in. There was a visual image of an 'N' I had before today; then of another 'N'; then I went through the list of types. 'American Typewriter' was present to consciousness for the briefest space of time in visual imagery. Then I visualized the name "Della Robbia" printed in small (lower case) letters. My attention turned from that to Clearface and I became aware of 'Clearface' in vocal-motor terms with a questioning accent. Then a visual image of a Della Robbia 'S' stood out in consciousness and seemed exceedingly familiar,—the serifs were just like those on this 'N.' I visualized both and said 'Della Robbia' instantly.

51. March 14, 1913. Della Robbia 'N,' 2 sec. (6th. presentation; second recognition.) "I noted during the presentation that the lines were of uniform thickness, and that the letter was large. That was followed by an intense strain in my vocal-motor apparatus, which meant that I knew the type and had a keen desire to say something, but I had absolutely no idea of what the name was. Then a visual image of the two 'l's' and the two 'b's' of Della Robbia rushed into consciousness and touched off my vocal-motor response 'Della Robbia.'"

52. April 24, 1914. Della Robbia, 'N,' 1 sec. (7th. recognition.) (Recognitions three to six were practically identical with that quoted above.) "I noted that the lines were uniform, and that the letter was large. First there was a sudden increase in my familiarity with a movement of my lips to say 'Bulfinch.' Then I had a visual image of two 'b's' and I immediately said 'Della Robbia.' Tension was instantly relieved."

*The Recognizing of Auditory Stimuli*

53. Jan. 17, 1913. 'Overture 1812' (Tschaikowsky), 10 sec. (3rd. presentation; 1st. recognition.) "When the music first began, I was aware of the character of the music as such,—it was fast and confused. I was conscious, too, that my attention was not very alert at this time. The next thing I perceived was the beat of that rhythm, this perception seeming to consist in an ability to anticipate the note in auditory fashion. A slight feeling of pleasure accompanied this. Then suddenly, I was aware of the *Aufgabe*, which seemed to be present as an effort of keener attention. Several successive visual images now came in; the first was the word 'Tschaikowsky'; next the word 'Danse Trèpak,' accompanied by a fleeting visual image of a Russian dance. There was now an increased familiarity which seemed to be associated with the image of a piece of paper localized off in space, but no word was perceptible on it. I thought I knew what the name was, however, this being present in a faint visual image of 'Tschaikowsky' off in the fringe of consciousness, located at the left. I was conscious of kinaesthetic sensations in the region of my mouth which was opened as if to say 'O.' Then suddenly I

found myself pronouncing 'Overture' and immediately 'Overture 1812' appeared upon my visual image of the piece of paper. My reaction then followed in mechanical fashion."

54. Jan. 20, 1913. 'Overture 1812,' 5 sec. (2nd. recognition.) "When the music began my attention was diffuse. I noted the accompaniment in the bass and it seemed slightly familiar. I found myself attending closely to those chords; I visualized them on a manuscript. At the same time, I recalled having heard such chords before,—this is an association of my manuscript image, with faint visual images of the situation here in this experiment on two former occasions. I then anticipated, in a clear auditory image, a phrase in the treble which is my criterion for this selection. When the anticipated phrase came, there was a relaxation, together with a visual image of 'Overture 1812' in your handwriting, a short distance out in space in front of my eyes."

55. Feb. 17, 1913. 'Overture 1812,' 3 sec. (3rd. recognition.) "The music at first did not bring any familiarity. I seemed scarcely to perceive it. Suddenly an awareness of the auditory impression rushed in upon me and I hurriedly went over the music I had already had, with auditory imagery of the air in the treble and of the accompaniment in the base. It was now very familiar. Vocal-motor image 'Overture 1812' followed by 'Tschaikowsky' rushed into consciousness instantly and I was aware of a relaxation of tension."

56. Jan. 20, 1913. 'Il Trovatore' (Verdi), 2 sec. (5th. recognition.) "My attention was keen when the music began. I was instantly aware of that chord which stands out in the bass, in four beats. Not more than two chords had been struck in the accompaniment before I anticipated the rest of the measure in vocal-motor auditory imagery. This made me absolutely sure that it was the 'Il Trovatore' selection, and all tension relaxed."

### *The Recognizing of Tactual Stimuli*

57. Nov. 21, 1912. 'y.' (This letter is represented by three raised dots, arranged in the form of an equilateral triangle, with its apex above a horizontal base.) 26.4 sec. (2nd. presentation; 1st. recognition.) "When I first explored the dots, the word 'new' came to consciousness in vocal-motor imagery. At the same time I was dimly aware of a number of past experiences with these letters,—'z' and 'c' appearing fairly clearly in visual imagery. I associated the stimulus with 'z' at first, visualizing the dots on the lower line as three in a straight row. Then I visualized the upper line of 'z' and explored for it with my finger. But I could find no upper line in the stimulus, and 'z' dropped out of consciousness. Next a visual image of a typewriter 's' appeared but it seemed new and strange and it in turn slipped out of mind. All the time my visual image of the arrangements of the dots was clearing up; when it became definite, a visual image of a type 'y' stood beside it and I reacted. Later I recalled going through this last process in learning it before."

58. Dec. 6, 1912. 'y,' 5 sec. (4th. presentation.) "With the first tactual impression, I visualized a triangle having two long sides, and a shorter one to the left. Then, a visual image of 'y' appeared as though superposed upon this triangle. This experience was rather unpleasantly toned, and the words 'more time' came to consciousness in vocal-motor imagery which, together with a more deliberate exploration of the stimulus, meant that I must examine it more closely.

My visual image of the group of dots became clearer, and my familiarity greater. My triangle became equilateral, with its base toward me; and a visual image of 'y' appeared beside my image of the dots. This, together with a memory image of myself sitting here before, completed my recognition."

59. Feb. 14, 1913. 'y,' 2.8 sec. (6th. presentation.) "On first getting the tactual impression, I had a vocal-motor image of 'v' with a visual image of these three dots arranged in triangular form, but no definite localization of the base or apex. This visual image, however, cleared up immediately and the base was toward me. Then a visual image of 'y' appeared and my attention turned to you."

60. Feb. 21, 1913. 'y,' 1.6 sec. (7th. presentation.) "I immediately visualized the stimulus as a triangle with its apex upward, and said 'y.' That was all,—no visualization of the type 'y' and no feeling of familiarity. Whole experience seems automatized."

### *The Recognizing of Olfactory Stimuli*

61. Jan. 24, 1914. Coast Violet, 9 sec. (2nd. presentation; 1st. recognition.) "It is familiar but I can not get its name. Attention lingered upon the actual quality of the sensation only for an instant. The sensation seemed to be of slight intensity and was visualized, at the opening of the nostril, as a little amorphous gas. Immediately following this came a series of visual and vocal-motor imagery. First the visual, in which I saw a portion of ground and on it was a plant just coming up, something like a skunk cabbage; its top was chopped off. Vocal-motor strain to say something and there was nothing to say. Strain was so intense that attention shifted for an instant to a visualization of my speech apparatus, with muscles in state of contraction. Only thing that came in was the word 'herb' repeated two or three times. General physical state of tension, localized in the speech apparatus, and about the face. These processes, together with the accompanying pleasant affective toning and the absolute predominance of this imagery,—the abandoning of everything else,—was the structure of this familiar toning. It was a one-level consciousness. The visual image of this stump and the vocal-motor 'herb' were so intense and clear and sudden that it made the familiarity a very intensive experience, though I can not name the perfume."

62. Feb. 7, 1914. Coast Violet, 15 sec. (3rd. presentation.) "I have not the slightest idea what it is. I visualized, in the fore-period, the processes of putting it up to my nose. I was aware of the *Aufgabe*, in auditory vocal-motor imagery: 'See if this is familiar.' The sensation first came as a strong, penetrating odor. It tickled the membranes of my nose and had a slight oily quality. The sudden attraction of attention to the quality and the persistence of that quality in the perception; a welling-up of pleasant affective toning, plus a mass of imagery of previous sittings which came in just like a flash; an actual memory-image of that same odor before,—all that stood out; and I interpreted the whole experience as familiarity with this odor. The word 'Quelques Fleurs' appeared in auditory imagery almost as soon as I perceived the pungent, oily quality, but I have never associated that oily quality with Quelques Fleurs. For a few seconds I was aware of the *Aufgabe* to try to get the name of it. This consisted in vocal-motor 'get the name.' Then followed series of visual images of bottles lying on the table; and snatches of auditory imagery: Rose, Coast Violet, Acme Violet,' but with faint olfactory



imagery of violet-like odors. Then it was all off, because I laughed. These images died away and no more came in and all I was aware of was strain and the occasional presence of the *Aufgabe* to find a name. Attention lingered on these processes for a while and then I said I had forgotten what the name of it was."

63. Feb. 28, 1914. Coast Violet, 13 sec. (4th. presentation.) "The odor immediately gave rise to memory-images of former sittings in this experiment. Its quality was unpleasantly pungent, and I experienced a motor tendency to recoil. Then I began analyzing the sensation, and noted that it seemed oily. I visualized it as something greasy on the inner membranes of my nose. The experience now became pleasant. There was a lot of ill-defined imagery of former experiences in this room,—visual, olfactory and auditory,—and I heard myself saying, 'Why, yes, I know that. What's its name?' There followed imagery of your voice saying 'Trèfle; Coast; Acme,' and others. Then came visual images of a big herb, like a skunk cabbage, growing out of the ground. Auditory imagery of the words 'herb' and 'Coast' persisted in appearing and I said: 'This must be the one.' Intense pleasantness now toward the experience, but immediately there appeared in consciousness simultaneously, visual imagery of many bottles, with attention moving from one to another, and an experience of doubt."

64. March 21, 1914. Coast Violet, 7 sec. (5th. presentation.) "I am not certain of my identification. Very little imagery. Nothing called up. I found my attention shifting from the bottle in my hand to the actual olfactory sensation itself. I found myself attending to a sort of pungency with a violet-like quality which came to me somehow associated with a visual image which seemed to be like a purple dome. The presence of a little off-shoot in this dome, with a kinaesthetic image of jumping back, was the structure of what meant to me 'there is a new factor which I did not distinguish before.' This intensive experience was followed by a faint visual image of a patch of ground with an herb coming up. Then that faded out and my attention was entirely taken up with the experimental surroundings."

65. March 28, 1914. Coast Violet, 14 sec. (6th presentation.) "It is as familiar as can be, but I can not be sure of the name. The first name that came was Coast Violet. Another possibility was Trèfle. Those were the two clearest ones. Very keen concentration of attention. Auditory *Aufgabe*: 'Now get the essence of this' with concentration of attention upon it. First I took a long, deep whiff, and I was distinctly conscious of doing it; it seemed to be an activity directed by the *Aufgabe*. Visual and auditory imagery followed in rapid succession, accompanying this deep inhalation. My attention went to the particular quality of the olfactory sensation; this was almost a visual and cutaneous thing. I had a visual image of a film, very thin and brownish, in the inner side of the left nostril. It was smooth and shiny and that with the peculiar contraction of the tongue in the back of the throat, and also a contraction of the diaphragm meant to me this pungent odor. But almost immediately following this there was auditory image 'pungent'; then attention shifted to another aspect of this quality. It seemed watery and almost gaseous; reminded me of illuminating gas. This was immediately followed by a visual image of a surface of the ground; saw lot of plants coming up; did not know what they were but I know now that they were skunk cabbages; saw them lying there among moss and wet leaves.

This was immediately followed by an auditory image, turning into visual, of Coast Violet. Then there was an experience of doubt. I could not seem to leave the problem. Memory images of past experiences persisted to demand my attention."

66. April 4, 1914. Coast Violet, 2 sec. (7th presentation.) "That is Coast Violet. At first I was not even aware that it was a perfume. I noted that the sensation was weak, and I inhaled deeply. Then I felt a sort of contraction in the back of my throat and tongue; this sensation seemed to extend to my diaphragm. Then I visualized an oily surface low in my nostril. The experience became very pleasant. Almost simultaneously my attention fastened on a visual image of a meadow, which seemed at my very feet, and in which were some hazy, ill-defined plants, which, I suppose, were my skunk cabbages. But instead of attending further to them, I found myself experiencing more intensive sensations in my stomach and diaphragm, and these were superseded by a visual verbal image of Coast Violet, the Coast appearing very prominently, written in white on a black background. I settled back in my chair with great satisfaction as I gave the signal-tap."

### *Observer V.*

#### *The Recognizing of Auditory Stimuli*

67. Dec. 9, 1912. 'Brautlied' (Goldmark), 20 sec. (2nd. presentation; 1st. recognition.) "The air seemed slightly familiar from the first. My familiarity with it seemed to consist in the clearness of my perception of the sound, together with kinaesthesia in my entire body,—swinging to the rhythm. An impulse to react, consisting of a series of tactual images of pressure on the key, was inhibited,—no name having yet come to consciousness. Then my attention was concentrated on the stimulus, and when that little "skip" in the music occurred, it brought a vocal-motor image of 'Bridal Song.' I reacted to that, and then ceased to attend to the thing."

68. Jan. 28, 1913. 'Brautlied,' 10 sec. (3rd. recognition.) "It was familiar from the first, the thing clearest in consciousness being my attention to the stimulus and my inhibition of a strong tendency to react." (The hand was poised over the reaction-key in a tense position.) "I visualized a threshold with a pair of feet in white satin shoes skipping over it. Then came a change in my kinaesthesia,—innervation in my feet and changed respiration. There was a pleasant affective toning and vague vocal-motor and auditory images of the act of describing something like this before. My reaction was followed by marked relaxation."

69. Feb. 25, 1913. 'Brautlied,' 5.4 sec. (5th. recognition.) "The familiarity came first as the memory-image of my kinaesthetic adjustment to this air,—that peculiar experience of thinking that you have done something years ago in exactly the same way in which you are doing it now. Just before I reacted, there was a visual image of a low door-step and a small white foot just going over it. This visual image was supplementary, I think, and more or less irrelevant to the familiarity itself, which consisted in kinaesthetic adjustment, followed by relapse of tension."

70. Feb. 25, 1913. 'Danse Trèpak' (Tschaikowsky), 2 sec. (5th. recognition.) "Immediately on noting the stimulus, my whole body seemed to sway in motor imagery, which was so clear as to seem

almost like actual movements. The facility and clearness of my perception of the music, together with this motor response, constituted absolute familiarity. 'Danse Trèpak' in vocal-motor terms immediately followed this experience."

### *The Recognizing of Tactual Stimuli*

71. Nov. 21, 1912. 'a,' 4 sec. (4th. presentation; 1st. recognition.) "There was a slight familiarity as soon as I got my tactual perception of the dots. I was impelled to count them, however. My attention was held involuntarily to that activity. I explored the dots one at a time. Then I got a visual image of two dots which seemed to be perfectly familiar, though no names presented themselves,—and, indeed, no name occurred until I actually said 'a.'"

72. Dec. 6, 1912. 'a,' 3 sec. (5th. presentation.) "I had an anticipatory visual image of two dots when my finger touched the first one. There was a vocal-motor image of 'a,' followed by a vocal-motor image of 'e.' My attention again fixed on the visual image of the two dots, and I reacted to the vocal-motor 'a,' which then became focal in consciousness."

73. Jan. 31, 1913. 'a,' 1 sec. (6th. presentation.) "The tactual sensation was immediately familiar,—the tension all over my body was instantly relieved. Then there came to mind a visual image of a large, black capital 'A.'"

### *The Recognizing of Olfactory Stimuli*

74. Feb. 23, 1914. Quelques Fleurs, 4.8 sec. (2nd. presentation; 1st. recognition.) "The first thing of which I was conscious, after sniffing the odor, was intensely pleasant affective toning. Am sure of quickened heart action and increased sensation of warmth, beginning about the middle of the torso and proceeding upward. Then the name 'Quelques Fleurs' came, visually, printed in violet colored, rather fancy letters. My attention was narrowed intensively, and when I reacted, there was a distinct sense of relaxation."

75. March 1, 1914. Quelques Fleurs, 6 sec. (3rd. presentation.) "I recognized immediately that it was a perfume, and very pleasant. This recognition and a feeling of confidence that I could identify it was present, I think, just in the manner in which I put the bottle up to my nose for a further examination of the odor. On getting a clear perception of its quality, the word 'Quelques' appeared in Japanese letters, on a large, cut-glass bottle, of which I saw only the neck and the stopper, which was frosted. Then I said 'Quelques Fleurs,—that's my favorite of all your perfumes.'"

76. March 3, 1914. Quelques Fleurs, 2 sec. (4th. presentation.) "A strong feeling of pleasantness accompanied the sensation. At first, my attention seemed very diffuse and I was aware only of the odor quality and of its great pleasantness. Then suddenly 'Quelques Fleurs' rushed into mind in vocal-motor imagery, and I knew this was a familiar odor, and reacted immediately."

77. March 22, 1914. Quelques Fleurs, 2.6 sec. (5th. presentation.) "That was an experience of immediate and direct assurance. Strong pleasantness was the first thing in consciousness, after a perception of the odor quality. A deep intake of breath followed, and a visual image of an advertisement for Quelques Fleurs which I saw in 'Life' last week. There followed an auditory image of my own voice, saying, as I then said, 'Now, you've got it in print, after all those

visual images you've had of it. *Quelques Fleurs.*' Then I pronounced it aloud. But all of this last was quite incidental to the recognition, which was complete when I took the deep breath."

### *Observer B.*

#### *The Recognizing of Auditory Stimuli*

78. Dec. 5, 1912. '*Il Trovatore*' (Verdi), 6 sec. (3rd. presentation; 1st recognition.) "The first note seemed to draw my body over toward it. I experienced a motor tendency to keep time, an innervation of the throat as in humming, and kinaesthetic imagery of swaying with the rhythm. Then I went back in memory, to my first sitting here, for a name,—this in visual imagery of the situation here. A momentary search, an attitude of listening, and '*Trovatore*' came to mind in auditory imagery."

79. Jan. 9, 1913. '*Il Trovatore*,' 3 sec. (2nd. recognition.) "That was familiar immediately; my attention was narrowly focused, my breath was held, and I was conscious of muscular tension all over my body. The whole experience was pleasantly toned. As the first note ended I had a flash of auditory imagery of the next tone, which was swamped immediately by my perception of the tone itself. Tension instantly ceased. The name which then came in auditory vocal-motor fashion added nothing to my surety."

80. Jan. 23, 1913. '*Il Trovatore*,' 1 sec. (3rd. recognition.) "The familiarity with that was perfect from the first perception. It seemed to consist in the clearness and the directness of the perception itself. There was the merest intimation of kinaesthetic imagery of swaying with the rhythm, followed by relaxation and turning to introspect."

81. Jan. 30, 1913. '*Rigoletto*,' 15 sec. (1st. recognition.) "My experience consisted of a perception of the tone quality and phrasing, together with a flash of visual imagery of a previous sitting in this room. I seemed to grope over in the direction of the table (where the experimenter always sat) as though to get the name from over there. My visual schema was present, too,—that image of four gray blotches descending obliquely toward the right, on a lighter gray background, and ending in a curving line. There was a motor experience also, in this schematic imagery. I seemed to move with the blotches and the curved line. When the phrase which fitted into this schema was played, the name '*Rigoletto*' sprang into consciousness, in auditory vocal-motor imagery."

### *Observer O.*

#### *The Recognizing of Olfactory Stimuli*

82. Jan. 31, 1914. Amyl Alcohol, 3 sec. (2nd. presentation; 1st. recognition.) "On perceiving the stimulus there was a recognition process which was very intensive; it seemed to be an organic drawing up in the upper part of the diaphragm, together with vocal-motor imagery of '*That's the one.*' Sensation seemed to grow increasingly intense; it seemed to increase way back in the back part of the nostril. Vocal-motor imagery '*alcohol*' came up and then vocal-motor '*Oh, I don't know whether it is Ethyl or Amyl alcohol; but I know the odor.*' That came in scraps like '*Amyl, Ethyl,—don't know; anyway, it's that one,*' which meant it was the one which

made me cough. Slight, pleasant affective toning accompanied by vocal-motor 'Well, I know that, but I don't know its name.'"

83. Feb. 7, 1914. Amyl Alcohol, 3 sec. (2nd. recognition.) "In the fore-period, I had a vague visual image of my hand holding the vial and it seemed to be a visual image of motion,—just a line very slowly drawn toward me. There was a very quick recognition process, though the name did not come to me. It consisted in a very clear and precise percept of the odor, accompanied by vocal-motor 'one of the old ones' and also 'one of those bad ones.' I could not name it. There was vocal-motor 'one of the alcohols?' Then there was a pause and during that pause the unpleasant affective toning continued. There was strain in the forehead. Vocal-motor imagery of 'what, which one?' There was no answer to that and the last thing in consciousness was that 'what, which one?' all accompanied by unpleasant affective toning."

84. Feb. 14, 1914. Amyl Alcohol, 1 sec. (3rd. recognition.) "On perceiving this there was unpleasant affective toning which lasted only a very short time. Then there was a quick change to a pleasant affective toning and a vocal-motor 'Oh, there it is,' and 'that is the bad one.' That completed my recognition. 'What's its name? Is it Amyl? Yes, Amyl,' followed, in vocal-motor imagery."

### *Observer S.*

#### *The Recognizing of Olfactory Stimuli*

85. Feb. 7, 1914. Cresol, 17 sec. (2nd. presentation; 1st. recognition.) "The very first perception was followed by vocal-motor imagery, 'It is either Phenol or Creosote. Now which is it?' This was followed very, very quickly, by clear olfactory imagery of creosote, which was apparently quite separate and apart from the actual olfactory sensation. There was a recall, also, of the characteristic mark or sign of the creosote odor,—the peculiar way it affects the lower part of the nose and persists. I was next aware of inhaling and of letting the gas out very slowly. This was followed by very pleasant affective toning and vocal-motor imagery of 'that's it; that's Creosote.' But I couldn't let it go at that. The adjustment was not complete. It was followed by another rise in the attention wave. I was aware of a visual image of a little square bottle on which was the word 'Mapleine'<sup>11</sup> and I was aware then of olfactory imagery of the odor of Mapleine. This imagery was broken into by a consciousness of self and of you and this consciousness of the conditions of the problem was further developed by the vocal-motor imagery, 'Well, she would not give me that kind of an odor.' Then my attention narrowed. I was aware of unpleasant affective toning, and of the following vocal-motor imagery: 'Oh, that's all right; that's creosote, all right. The mapleine is an artificial preparation and the chances are they use creosote in making up that Mapleine.' Then my attention shifted and I turned to introspect."

86. Feb. 14, 1914. Cresol, 8 sec. (2nd. recognition.) "The perception was complete at first and persisted in great clearness. The first thing I was aware of was the vocal-motor phrase 'maple sugar.' Then there was a very brief series of olfactory and visual images, connected with the experience I had here the other day when that

<sup>11</sup> This observer reports that Mapleine is a synthetic substitute for maple syrup; it is characterized by a faint odor of creosote or tar.

maple sugar characteristic was assigned to the creosote. I think it was mostly in olfactory imagery. I had very fleeting visual images of the bottle marked 'Mapleine,' and I was aware of attending to a particular part of the stimulated nostril,—the lower part. Then just the words 'maple sugar,' followed an image of the Mapleine bottle and a few of the details of the former experience here. Then the word 'creosote' came automatically."

87. March 7, 1914. Cresol, 6 sec. (3rd. recognition.) "I was aware of making a definite motor adjustment to the sensation,—of breathing easily, in shallow breaths, and of attending to the lower part of the olfactory membrane. Then there was a definite awareness of attending to the persisting sensation and waiting for something to happen. Finally the words 'Oh, that's creosote,' came in vocal-motor imagery, and my assurance was complete. Later I had olfactory imagery of that characteristic maple sugar quality and then my attention was directed to an image of this Mapleine bottle. I said, 'That's creosote, all right,' and was aware then of general relaxation."

88. March 14, 1914. Cresol, 2.4 sec. (4th. recognition.) "That is Cresol. I was aware that I was inhaling and then exhaling, that the lower part of the olfactory organ was being stimulated, and that the sensation persisted after the bottle was removed. Even after I had breathed through my nose two or three times I was aware of the stimulation still there. This persistence of the sensation seemed to be the antecedent to the coming in of all the experiences I have had where the persistence was a factor. Almost the first one that came was the maple sugar. I had a very indefinite, inadequate image of a lump of maple sugar. I was aware of the persistence of the odor after I smelled it. This meant to me that this was undoubtedly the Cresol."

89. March 21, 1914. Cresol, 2 sec. (5th. recognition.) "I attended first to the characteristic quality of the stimulus. It is a queer smell quality,—coming intermittently, and affecting the lower part of my nose like little particles of dust. Its persistence attracted my attention and I was aware of vocal-motor imagery, 'that's Cresol.'"

*Observer Bd.*

*The Recognizing of Olfactory Stimuli*

90. Jan. 24, 1914. Halycon Rose, 4 sec. (2nd. presentation; 1st recognition.) "On smelling that, I had a reinstatement of the mental experience which I described at the last sitting,—an association with the odor of wild roses, and vocal-motor 'briar.' The whole thing seemed very familiar. After I reacted, I had a visual image of the part of the wild rose that is left when the petals come off."

91. Jan. 31, 1914. Halcyon Rose, 17 sec. (3rd. presentation.) "I don't know what it is. That was rather a remarkable experience. With the first inhalation of the right nostril I got the quality of the odor very distinctly but was also conscious of unfamiliarity, uncertainty. I began a second inhalation, and suddenly, during the course of that inhalation there came up a feeling of familiarity, but it was a feeling, largely organic, in the upper part of the chest and upward,—not very intensive and not definitely localized, lasting only for a brief time. With that came the vocal-motor imagery 'going to get it,'—a fragmentary sentence which indicated that I was on the point of getting the identification. Then feeling of familiarity disappeared and attention lapsed."

92. Feb. 7, 1914. Halcyon Rose, 9 sec. (4th. presentation.) "Perfume" came immediately in vocal-motor terms, and with that a feeling of familiarity which I should say was largely organic in the upper chest region; pleasant. 'Quelques Fleurs' was present in vocal-motor terms, but I was not at all sure of my identification. Then I smelled it again. Just at the end, I got that peculiar briar quality that I spoke of the other day and immediately 'briar' came, vocal-motor, and 'rose,' vocal-motor. I am quite certain that that was rose."

93. Feb. 14, 1914. Halcyon Rose, 8 sec. (5th. presentation.) "Almost with the beginning came the word 'p-e-r-r-r-fume,' drawled out in vocal-motor terms, with the inhalation. I felt myself pronounce this in that long drawl. Not aware of any specific feeling of familiarity. Along toward the finish I got a slightly changed olfactory quality; and the word 'briar' appeared in auditory terms. I am surprised at the amount of auditory imagery this morning because it is so unusual. My identification was not yet complete. I was led to suspect that it was going to be rose because of that 'briar.' Then I smelled it again. I approached that with a very definite *Aufgabe*, vocal-motor terms: 'Is it rose?' Found myself very tense and very alert and active, examining my experience very closely; but I was disappointed,—the word 'rose' never occurred after the *Aufgabe*, neither did 'briar,' simply a fairly intensive olfactory quality and not a single name associated. My experience was,—starting out with this *Aufgabe*,—a very, very definite olfactory quality, a certain tenseness, a certain disappointment, and an uncertainty."

94. Feb. 21, 1914. Halcyon Rose, 4 sec. (6th. presentation.) "The instant I began to sniff came 'perfume' and feeling of familiarity,—very intense. Then it disappeared and this vocal-motor imagery came in automatically: 'It must be briar; she has given me Quelques Fleurs; she has given me Coast Violet; she has given me Acme Violet.' That was all very syncopated. Meanwhile I did not attend to the odor at all. As yet I had not got any qualitative rose odor at all, but after that thing that I call a logical inference, suddenly there welled up that peculiar unmistakable briar quality, and I knew then that it was rose. I said 'rose' and then turned to introspect."

95. March 7, 1914. Halcyon Rose, .8 sec. (7th. presentation.) "With the very beginning of the sniff, I had a vocal-motor image of 'rose.' I continued to sniff, and the quality changed to that briar quality. 'Briar' came in vocal-motor terms, and my surety increased. The whole experience was slightly pleasant, though not markedly so."

96. March 14, 1914. Halcyon Rose, .8 sec. (8th. presentation.) "'Rose' came immediately, in vocal-motor terms, and I felt absolutely certain of my recognition of the odor. I hesitated for a moment before tapping, as though expecting that briar quality to come, but it didn't. This was simply a case of actively waiting for the quality to appear. Meanwhile, I had a distinct olfactory image of that briar quality. Yet, I was aware throughout of the actual olfactory sensation which this stimulus brought. I think it was a case of rapid alternation of attention from sensation to image. There was no affective toning."

## B. The Functional Components of Recognition

a. *The rôle of the Aufgabe.* The essence of the process of recognition is function, not content. It is true that the content

of the act of recognizing exhibits certain interesting and important characteristics which bear a direct relation to the degree of familiarity which the object recognized possesses. But, given the same images, sensations and affective elements, together with a different *Aufgabe*, a changed order of events and a new reaction to them, and the result will be a mental process of an entirely different nature. It is the pattern into which the warp and woof of sensation, image and affection is woven which gives its characteristic nature to the cloth we call recognition; and this pattern is dependent upon the behavior of attention, directed by the *Aufgabe* to recognize.

Although the *Aufgabe* was not always explicitly present, and although it was seldom focal, its influence on the cognitive experiences of our observers was evident throughout. When the observer was explicitly aware of the task to recognize, this awareness most frequently took the form of vocal-motor processes such as: 'Now, get this'; or 'What is this?'; or 'Is it De Vinne?'; or, more frequently, a telescoped and fragmentary representative of such a consciousness,—often a mere beginning of the word 'What?' Consciousness of diffused bodily strains, an awareness of the hand poised above the reaction-key, a tension in the vocal-motor organs,—as though they were about to pronounce a name,—these structural components were again and again described by our observers as constituting their task-consciousness or as partially composing this consciousness. Certain other structural elements appeared in individual cases. For example, *W.*, on several occasions, described an *Aufgabe* whose structure was a 'visual image of the experimenter, wearing an expectant expression of countenance.' Again (53, p. —) he was aware of his task as a consciousness of increased effort to attend narrowly. In many cases the recognition *Aufgabe* was not clearly present in explicit terms, but appeared rather in the attitude which the observer took up toward the stimulus presented. In such cases it was implicit in the sequence of noting, comparing, contrasting and the like; and in the release of tension which completed the act of recognizing, and which constituted an attitude that the task had been accomplished,—an attitude or a mode of behavior too implicit to be called an awareness.

The *Aufgabe*, explicit or implicit in the activity itself, serves to direct the attention to the characteristic feature of an object presented for recognition; to call up, in certain cases, associated imagery or other processes; to institute certain acts of



comparing, weighing, accepting, discarding, and the like; in a word, to direct the entire course of the activity.

Such dispositions, once set up, were exceedingly tenacious; and they persisted in many instances, notwithstanding opposite instructions on the part of the experimenter. Wide individual differences in such cases were brought out in a variant of the experimental procedure, where the observer was instructed not to search for the name of the stimulus presented, and not to attempt to recognize it,—but merely to perceive the object exposed and allow an entirely passive consciousness to run its course. Two of our observers found themselves utterly unable to follow this instruction. A strong disposition to identify the stimulus directed their attention in the customary channels, in spite of their attempts to inhibit this activity.

One of the most striking manifestations of the functioning of a deep-seated *Aufgabe* occurred in a still different variant of this experiment, where the letters presented in the recognition series were printed in red ink instead of in the usual black.<sup>12</sup> *Fn.* entirely failed to apprehend the difference in color in all of the ten exposures made in this manner. He even referred to his 'noting the blackness of the lines' in his introspection on his ninth experience with this series. Evidently, he had set up for himself an *Aufgabe* of noting the form of the exposed letters, and this operated so potently as to exclude altogether a perception of their color.

The effects of the *Aufgaben*, explicit or implicit, are discernible in every recognitive experience described by our observers. In the case of but slightly familiar material, these dispositions served to direct the attention to characteristic features of the stimulus, stimulating the revival of former experiences; they initiated various acts of comparing, contrasting and the like, which culminated in a complete change in the direction of the attention as the act of recognizing was completed. In intermediate stages of recognitions, the operation of the *Aufgabe* is similar to that just described, save that processes between the initial perception and the appropriate reaction are here telescoped or attenuated. At the final stage, the essential characteristic of the recognitive experience consists in the fact that the perception occupies the focus of attention, whence, under the direction of the *Aufgabe*, there is initiated an immediate and appropriate reaction, such as speaking the name of the object recognized, tapping a reaction-key,

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<sup>12</sup> This variant was tried on all the observers at the close of the series of visual recognitions.

or making any other bodily adjustment which the circumstances render appropriate.

These processes are followed by a change in the state of the attention. This change consists essentially in a change in the processes which occupy its focus; the observer may turn from the consideration of the stimulus to an introspection of the processes involved in its recognition; or he may abandon the features of the object which have been focal in his activity and note others; or he may cease altogether to employ himself with the stimulus presented or the problem involved, and enter on a wholly different set of activities. The nature of the attention, as well as its direction, undergoes a variation in the moment of identification; *i. e.*, whereas it has been in a state of rapid fluctuation, over a wide range of activities, in that period of relaxation which crowns the recognition, it is now narrowed to a point of exceeding clearness and intensity. The direct product, then, of the awareness of the task is the behavior of the attention. We proceed to a discussion of this fundamental aspect of the experience of recognizing.

*b. The behavior of attention.* A survey of the experimental data can not fail to establish the fact that the attention exhibits a strikingly characteristic mode of activity during the process of recognition. When one compares a body of introspections such as these experiments have furnished, with introspections on other so-called higher mental processes, such, for example, as judging, willing, abstracting and the like, one is struck by the fact that the contents of all these processes bear a marvelous similarity. It is possible, however, to detect certain differences in the manner in which the content elements of the various processes function.

The introspections of our observers furnish evidence of the fact that the temporal sequence of the processes which follow a perception, their relative clearness, their duration, and the nature of the attention bestowed upon them, manifest certain characteristics corresponding to different degrees of familiarity or mechanization of the process of recognition. These phenomena are the very essence of the recognitive experience. A consideration of the mental experiences described by each of our observers will disclose the facts which have led us to this view.

*a. Case Studies of Learning and Recognizing.* The results of these investigations show that there is a most intimate relation between the learning procedure and the nature of

the subsequent recognition of the learned experience. In order, therefore, to account for the characteristics of any given act of recognition, it is necessary to study its genesis in the learning process. Such processes exhibit interestingly diverse features in different individuals; and it is with the hope of contributing somewhat to the study of individual differences, as well as to the envisagement of the recognitive experience in process of formation and development, that the following case studies are here appended.

*Observer F.*

*Visual series:* In learning the forms of the different faces of type, *F.* invariably fixed his attention on one or two features prominent in the appearance of each, and described them, in vocal-motor terms, throughout the entire exposure-series.

This description was usually accompanied by the name of the type in question,—the whole phrase being sometimes actually whispered, but oftener appearing in vocal-motor imagery; *e. g.*, while learning Clearface type,—a face larger than the others employed, and composed of uniformly heavy lines,—*F.* constantly repeated 'big, thick Clearface' in vocal-motor imagery. His position was tense, and gave evidence throughout of his excitement and anxious desire to master the material. The introspections (quoted pp. 324f.) show how persistently this procedure influenced *F.*'s later experiences with this face of type. Another striking instance of such influence occurred in the case of the Bulfinch type. During its initial learning series, *F.* had repeated to himself, 'This thin, too. Della Robbia thin, *same*; Bulfinch thin, varied,'—the obvious meaning of these syncopated phrases, in this setting, being that while the Della Robbia type was composed of fine lines, they were not of the same width throughout any entire letter, while the lines of each letter of Bulfinch were uniformly narrow. Until the very last, this firmly fixed association with Della Robbia persisted. It was the only face with which *F.* ever confused Bulfinch; and long after it ceased to confuse him, the association retained its accustomed place in the sequence of experiences aroused by the Bulfinch type stimulus. In his initial recognitions, *F.*'s attention first seized upon the characteristics of the letter displayed for recognition, and then turned to an active, excited process of describing these characteristics in vocal-motor terms, with momentary pauses after each repetition. If the name of a 'face' of type appeared in consciousness, he attended closely to it; the verbal characterization and the name were then

repeated in conjunction. If the excitement abated, and if his attention abandoned this procedure, recognition was experienced, and he reported his identification with certainty. In cases where no such name appeared, the observer was conscious of an active search for one (1 and 2. The second of the introspections here quoted is typical of *F.*'s method of comparing various combinations of vocal-motor cues). In these initial recognitions, *F.*'s attention was occasionally occupied with a recalled visual image of other letters, which he compared or contrasted with the percept which he was endeavoring to identify.<sup>13</sup>

As the material became more and more familiar during the successive presentations, processes of comparing the percept with reproduced imagery gradually decreased in number, so that in the several phases of what may be termed the intermediate stage in the mechanization of a recognition, such comparisons appear less and less frequently. In the last of these phases,—a level at which the recognitions may be called 'final,'—*F.*'s attention seized immediately upon the characteristics of the letter displayed, and then turned to a vocal-motor image or verbal characterization, plus the name, *e. g.*, 'funny—Bulfinch.' The problem was then abandoned altogether.

At each of these levels, *F.* was aware of the fact that the stimulus was familiar. Introspectively, he could not abstract this awareness from the succession of processes which appeared in each case. He was conscious of the longer duration of the activities preceding the signal reaction in those cases where he had a consciousness of familiarity.

In the mechanized stage, where no "feeling of familiarity" was aroused, the succession of component processes was still further syncopated; here the appropriate motor response followed immediately upon his attentive perception of the stimulus. No awareness of familiarity or of 'surety' occurs in introspection 4 (p. 325). The distinctive awareness of "known," usually called the "feeling of familiarity," disappeared as the reaction to the stimulus became mechanized. On being asked, in several such cases, if the letter seemed

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<sup>13</sup> An illustration of this procedure, which, however, was not typical for this observer, is furnished by the following introspection: March 15, 1913. Bulfinch 'F.' "I was immediately aware of vocal-motor imagery: 'It is that funny one, the last one,—funny Bulfinch.' But just before that last phase came in, I had a clear visual image of Bulfinch 'F' and 'E' with their characteristic points, the 'E' being especially clear. Then I was again aware of the exposed letter and I reacted with certainty. Release of tension."

familiar, *F.* replied, 'There wasn't time. I just reacted automatically. Afterwards, I recalled other things about it, and it seemed very familiar.'

*Auditory series:* *F.*'s method of learning the airs played to him was distinctly different from that which he employed in mastering the visual material. First of all, his attitude was characterized by less strain and anxiety. He was confident of his ability to perform the task set creditably. This statement is based on data of two kinds: 1, the observer's overt behavior, which was marked by an easy, relaxed sitting position, a serene and often smiling countenance, and an absence of verbal or other expressions of discouragement; 2, his introspections, which show that instead of actively seeking associations between the music and its name, he usually listened with enjoyment, often tapping his foot, swinging his arm, or otherwise imitating the rhythm, and frequently humming along with it. The humming was usually in imagery,—vocal-motor and auditory,—though actual innervations were sometimes noted. Visual images of singers, orchestra leaders, dancers and, still more rarely, of musical instruments were aroused from time to time; but though these played a small part in the initial recognitions, they quite disappeared before the mechanized stages had been reached. In learning the selection whose recognitions are quoted (p. 325), *F.* invariably associated with certain bars of it, the words 'Hail, Pennsylvania,'—words which occurred, with the same air, in one of his college songs. As his introspections show, this association invariably played a prominent rôle in his recognitions of the selection.

*F.*'s initial recognitions of these airs were characterized by an attention which vacillated between the stimulus itself, and imagery of humming the air, or pronouncing its words or name. A consciousness of bodily tension which finally gave way to relaxation, constituted the background of all these experiences. A reduction and telescoping of these processes, and an abbreviation of their duration mark the intermediate stage; while in his final recognitions, *F.*'s attention shifted instantly from the perception of the stimulus to some appropriate motor response, which, in turn, was followed by a sense of relaxation.

*Tactual series:* In trying to learn the New York Point letter symbols, *F.*'s attitude was very like that which was present in visual series. His attention was narrow, his position alert and tense. His procedure consisted in passing his finger-tips

repeatedly over the group of points composing a given symbol, meanwhile repeating, either in vocal-motor movements or imagery, the name given to the stimulus by the experimenter. The number and arrangement of the points came to his consciousness in terms of tactual and kinaesthetic sensation. He very rarely visualized these stimuli, and visual imagery turned out to be an almost negligible factor in his subsequent recognition of them.

On perceiving the general form of the tactual material, *F.*'s attention, in his initial recognitions, turned to imagery (usually tactual, though sometime visual) of groups of dots, which he proceeded to compare with his present percept. This first stage was never of very long duration, and soon gave place to a type of consciousness in which *F.*'s attention turned directly from the perception of the stimulus to the name which had been associated with it in the past. A consciousness of strain, followed by distinct relief, completed the experience.

*Olfactory series:* *F.*'s dependence upon vocal-motor cues was strikingly shown in his procedure during the learning of the series of odors. His behavior gave clear evidence of his firm belief that he would succeed very indifferently, if at all; and his introspections give ample testimony of this disbelief in himself. Throughout the presentation of any odor for learning, he sat tense and sniffed vigorously at the stimulus, meanwhile repeating its name, and usually its striking characteristics over and over, in vocal-motor terms,—usually imaginal. For example, in learning Ethyl alcohol, he characterized it thus: "Cool; tickles in *nose*, not *throat*; Ethyl," and this he repeated throughout the exposure. Visual imagery scarcely occurred during this entire series.

The olfactory material furnished an excellent means of observing the behavior of *F.*'s attention in attempting to learn and later to recognize stimuli which had little intrinsic interest for him,—stimuli of a modality in which he believes himself poorly endowed and which furnishes him with few, if any, images.

His initial recognitions describe an awareness of attentively hurrying from the sensation under investigation to repeated reviews of series of vocal-motor images (15). His recognition seemed to depend on the reinstatement of just the proper sequence of images,—that sequence which had been adopted during the learning of the odor in question. This succession of experiences became somewhat syncopated as the odor grew more familiar; but its essential elements remained the same

throughout initial, intermediate and final recognitions. In the introspections quoted (pp. 326f.), *F.*'s characterization of the quality of the odor as 'cool in the nose,' continued to arise in consciousness as the initiator of his identification of this odor. The culmination of this experience of familiarity immediately occurred in the appearance of the odor's name, and *F.*'s reacting signal.

*Observer Fn.*

*Visual series:* In learning the visual material, this observer employed visual imagery chiefly, with vocal-motor as his only other imaginal content. During the exposure of the letters of the first type given him for learning, *Fn.* busied himself in noting the form of each letter, frequently calling up a visual image of some letter which had already been noted. When the series had been exposed as far as the letter *T*, he was aware of vocal-motor imagery of saying, 'I shall call this the ordinary type,—the most natural one.' As other faces of type were shown, *Fn.* constantly found himself calling up images of the letters in faces of type previously exposed, and instituting comparisons between these images and his present percept. He arranged the names of the five types in a visual schema, reading from left to right, in the order in which they had been first presented for learning; and most of his identifications were made by referring, to a given place in this schema, the letter which had been exposed for recognition. The schema was visualized, usually two or three feet to the left of the observer's eye; and the place in it which the type under consideration should occupy, stood out more clearly than the rest of the image. In his final recognitions, either an identification like the one just described took place, followed by the appearance of the name in vocal-motor imagery; or else the name of the type, in vocal-motor imagery, followed immediately upon the visual perception, which was marked by its ease and rapidity.

In the initial visual recognitions of this observer, his attention was attracted first to the size and general form of the letter; then to a rapid series of processes which had to do mainly with comparing and contrasting his present percept with certain images. A very characteristic procedure consisted in visualizing the letter in a definite place in his schema (described above), and then in making an immediate judgment as to whether or not the letter seemed to belong there. Very often, too, *Fn.* called up experiences of former sittings, and proceeded to compare the images thus aroused with the

percept now under consideration. He described the behavior of his attention as being 'sometimes active, sometimes passive,' *i. e.*, he believed he was sometimes aware of an activity and seemed to be directing the course of the processes which he experienced, while at other times he seemed rather a passive spectator of the pageant of his own ideas.

In later recognitions, processes of recall and comparison became fewer, until finally they altogether failed to appear. His attention in these cases behaved in one of two ways: after first seizing upon the characteristics of the letter displayed, it turned instantly either to a definite part of his visual schema, or else to a naming of the type,—the name appearing in vocal-motor imagery or in actual vocalization. The introspections quoted on p. 327 illustrate the changes in the behavior of the attention of this observer throughout the progressive mechanization of his recognitions of one of the faces of type.

In its final form, *Fn.*'s recognition was characterized by what he calls 'its ease and readiness' (21). Such expressions are abundant throughout his introspections, and appear always to signify an explicit mental content,—not an inferred datum. In other words, *Fn.* seemed to attend to the facility of the entrance of a percept into consciousness, as well as to the length of its persistence therein, as attributes of that percept,—components of the total experience highly important to the consciousness of familiarity.

*Auditory series:* *Fn.*'s learning of the musical selections was an exceedingly active process, rich in sensory components. Floods of imagery, visual, auditory and motor, made their appearance in consciousness. *Fn.* attends to music very analytically; he finds himself noting the phrases in auditory and motor terms, and even in visual imagery of the musical score. He frequently anticipates the phrase or phrases which are to follow.

In his initial recognitions of the auditory stimuli, also, *Fn.*'s attention was very actively employed with images, chiefly visual and auditory, having to do with former hearings of the airs, as well as with visualizations of the present auditory impressions (22). These visualizations consisted in various schemas, in gray symbols which corresponded spatially to the tonal rise and fall of striking phrases in the music. They were often ill-defined and shadowy, yet played a prominent part in *Fn.*'s earlier recognitions of these airs. If the visual schema aroused in consciousness by the selection heard was the one formerly associated with that air, a certain bodily



reaction took place,—the pronouncing of the selection's name; a tap on the reaction-key; relaxation of tension,—any or all of these. But, if some other visual schema arose before his mental vision, these 'familiarity reactions' did not follow, and no certain recognition ensued. In the experience quoted above, *Fn.*'s recognition of the air 'Brautlied' awaited, for its culmination, the repetition in consciousness of the motor reaction (looking in a certain direction) which had formerly been a part of his experience with this air.

Visual schemas played a lesser part in the intermediate levels of his familiarity with this material. In the series of recognitions quoted on pp. 327f., no visualization occurred after the first recognition. In other cases, however, the schemas persisted for various periods of time, sometimes becoming gradually less clear, and sometimes suddenly dropping out of the experience altogether. Revivals of past experiences and comparisons of them with the present stimulus were common at this stage of the experiment.

The final type of auditory recognition described by *Fn.* consisted in a rapid attentive perception of certain characteristics of the stimulus, followed immediately by a unique bodily reaction—a pervasive thrill, which seemed to be essential to his consciousness of familiarity (24) and to which he seemed always to attend.

*Tactual series:* In learning to recognize tactual stimuli, *Fn.* had recourse chiefly to visual imagery. His method consisted in obtaining a clear tactual and kinaesthetic perception of the number and arrangement of the dots, and then in visualizing that arrangement. Very little vocal-motor or auditory imagery was reported in any of his introspections on the learning of the letters, although the former made up a large part of the structure of his recognitions later on. *Fn.* reported more tactual and kinaesthetic imagery in his recognitions than did any other observer. He frequently remembered his former experiences with these letters in terms of kinaesthetic and tactual imagery; and, employing these same terms, he frequently anticipated the sensation which was about to be experienced when only a part of the stimulus had as yet been explored. Visual imagery is the most important structural component in the recognitive consciousness of this observer.

In his recognitions of the tactual material, we find *Fn.*, in the initial stages, attending actively to comparisons and contrasts between his perception of the presented touch symbols, and his revived imagery, both tactual and visual, of these experiences (25). Here again, visual schemas appear, in

which the letters were arranged in the order of their first presentation in the learning series. If, on the appearance of the visual image of such a schema, a given portion of it claimed his attention and stood out clearly, *Fn.* localized a visual image of the symbol which he was actually examining with his finger tips, in that portion of the schema; and his consciousness of familiarity was then intensified by an appropriate motor response.

As in the case of the auditory material, the visual schemas dropped out during the intermediate stages of mechanization; and thereafter his attention was less and less occupied with revivals of past experiences, though such processes still were a part of the recognitions at this level (26, 27).

Even in its final, or most nearly mechanized form, *Fn.*'s perception of a touch symbol was always attended by a visual image of the letter which it represented. In his recognitions of exceedingly familiar touch sensations (28), no revival of past experiences appeared. He invariably reported that he attended merely to some characteristic quality of the stimulus, to an awareness of the rapidity with which it came to consciousness: the 'permanence' of the character noted; and to the diffuse bodily thrill which ensued.

#### *Observer Fs.*

*Visual series:* *Fs.* employed both visual and vocal-motor imagery in her learning of these faces of type, often calling up visual images of other letters as the alphabet was being presented, and frequently characterizing letters, in vocal-motor imagery, as 'heavier,' as 'too fine,' etc. Her characteristic motor tendency in dealing with new material,—that is, her tendency to trace its form with the tip of the tongue against the back of the teeth,—was present in many cases.<sup>14</sup> Revivals of that tendency were observed in many recognitions of the same material. But by far the majority of all her recognitions included a reference to a visual schema which *Fs.* had built up during the first sitting. She described it as "five gray blotches which might be letters, located in a vertical column. They represent for me the names of the types in the

<sup>14</sup> The following introspection shows this process operating in a very interesting fashion. Bulfinch type had just been displayed in a learning series. "First I noted that this was a heavier type than the last. Then, with a great deal of satisfaction, I noted that I was getting a series of letters which had straight lines, of which the one on the right was always heavier. This came to consciousness in tongue movements. My tongue outlined the light lines with a slight pressure against the teeth, and then the heavy lines with an energetic stroke."

order in which they were presented the first time (Della Robbia, Bulfinch, American Typewriter, De Vinne, and Clear-face). When the first place demands my attention and grows clearer, the name 'Della Robbia' appears in consciousness, immediately, in vocal-motor terms, etc." This schema became much less detailed in the later recognitions, where, indeed, it sometimes failed to appear.

When letters which had only a slight degree of familiarity were presented to *Fs.* for recognition, her attention busied itself first with a minute scrutiny of each formerly noted characteristic of the letter; next, it usually turned to an attempted recall of each face of type which had been employed in the experiment. This recall sometimes took the form of attending to vocal-motor imagery of the name of the type-face; but the moment the name appeared in consciousness *Fs.* found herself attending to the appropriate place in her visual schema of the five gray blotches, each associated with the name of one of the faces of type (29). She frequently visualized some letter of each type in its proper place in the schema, and compared its size and characteristics with those of the letter actually displayed for recognition. Sometimes the visual schema appeared in consciousness immediately upon the noting of the letter displayed; and then her attention turned to a naming of the various parts of the schema. So long as her attention was occupied in this manner, *Fs.* was uncertain of her identification of the letter under consideration; but when this activity ceased, when her attention paused for a perceptible time on some one position in the schema or on some one name, which then became clear, a final turn of attention from the problem served to make the recognition complete and certain.

As the material became more familiar, there were naturally only two or at most three possible identifications which engaged the observer's attention. There was often a confused state, a consciousness of strained activity, with some recall and comparison. Sometimes a number of images, more or less clear and detailed, occupied her attention momentarily, only to drop out of consciousness when that insignificant pause in the activity initiated her identification (30).

Finally, when perfectly familiar material was presented, *Fs.* found her attention leaping from a group of the main characteristics of the letter,—or from a perception of the particular feature which was for her a cue,—to the name of the face of type to which it belonged, or to the tap which signified her identification of the letter. The introspections (quoted

on p. 329) illustrate the typical behavior of the attention in the case of this observer, at various stages in the experiment, with visual material.

*Auditory series:* In the case of the auditory material, the same characteristic differences appeared in the behavior of her attention when stimuli of different degrees of familiarity were presented. When relatively new airs were played, *Fs.* found that she had attentively compared or contrasted the auditory impression with images of airs formerly heard. Visual schemas (38) and other visual images were abundant. Various names of selections played during the experiment came to consciousness, some appearing only to disappear immediately, other persisting and claiming the attention for a longer or shorter time. The turning of her attention from a name to a motor reaction (either of pronouncing it, or of tapping the key) completed her act of recognizing the air. The intermediate stages of familiarity were marked by syncopation of all these processes.

In her final recognitions, *Fs.* was conscious of an attentive noting of the stimulus, followed by an awareness of rhythmic kinaesthesia, variously localized, and an immediate tendency to tap the reaction-key. The manner in which her attention turned from clear perception to appropriate reaction, with only the slightest pause or inhibition, constituted her consciousness of familiarity with the given air (37, 39). When absolutely no pause of process intervened, the characteristic 'feeling of familiarity' did not arise.

A striking confirmation of the dependence of recognition on a particular attention-sequence is found in the behavior of this observer's attention on several occasions when new airs were introduced in certain control experiments. For example, her awareness of unfamiliarity with a portion of *Traviata* (40) consisted in a persistence of attention to the stimulus and a vocal-motor auditory 'new.' Rhythmic kinaesthesia was present, and even an automatic reaction-tap; but there was no recognition *Aufgabe*, and no characteristic shift of attention. In other words, her attention had not behaved in the manner customary to the act of perceiving old stimuli, and the experience of non-recognition ensued.

*Tactual series:* Similar functioning of the attention marked various stages in *Fs.*'s consciousness of familiarity with the tactual material. In learning the touch symbols, *Fs.* made constant use of visual and vocal-motor imagery, actually visualizing the points as raised dots, which seemed to appear

on a white background.<sup>15</sup> She frequently visualized, also, the letter which a particular combination of dots represented, this image usually appearing in the form of a printed letter, just above her visual image of the dots themselves. The name given the stimulus by the experimenter was repeated by *Fs.* in vocal-motor imagery; and frequently some characterization of the arrangement of the points came to consciousness in the same terms.

Another learning method in which a motor cue was the essential feature is illustrated in her experience with the symbol for "f," which consisted of three dots in a horizontal row. She associated this with the deaf and dumb manual symbol,—passing one palm over the other. Motor imagery of this gesture was always a component of her experiences with this particular symbol.

In the initial stages, her attention was actively concerned with memory imagery, as well as with the characteristic features of the present stimulus. The consciousness of strain and of activity she called 'a persistent whatness'; it initiated her awareness that the stimulus was one with which she had previously dealt (41). This plan of active searching, image-evoking, etc. (42), became less prominent as her familiarity with these tactual letter symbols increased. Motor cues became more important in the intermediate stages; and finally, in the most nearly mechanized recognitions obtained in this series, her attention turned instantaneously from an active noting of the stimulus to some representative idea (visual or motor) of its form, and then to an appropriate motor reaction (43). The latter consisted sometimes merely in the signal-tap; but more often it took the form of a pronouncing of the name of the letter whose symbol was being investigated.

*Olfactory series:* When she was given an odor to learn, *Fs.* was entirely absorbed, at first, in attending to its smell quality. She endeavored to single out some unique characteristic which should serve later as a means of identification. This procedure she described, again and again, as consisting in repeatedly inhaling deep draughts of the odor and attending narrowly to its qualities. She often visualized the odor's name (which was pronounced aloud by the experimenter); and in case of what she called "flower perfumes," she frequently had visual images of flowers, perfume bottles, etc. Occasionally, the learning consisted merely in associating the

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<sup>15</sup> These letter-symbols had been presented only in tactual fashion. The observer was blind-folded throughout, and she never at any time saw the group of dots.

vocal-motor auditory image of the name directly with the odor quality. In some of her early experiences with the "non-flower odors," she constructed a visual schema, not unlike that employed in her visual series, in which each gray blotch was associated with the name and the odor originally presented in an order corresponding to the series number of the blotch in question.

Her overt behavior suggested that she considered the game an interesting one, and that she viewed her own performance quite objectively. There were no signs of discouragement, impatience or anxiety.

In this observer's initial recognitions of the odors learned in this experiment, her attention was divided among many claimants. The first perception of the stimulus seemed to initiate a flood of recalled experiences, chiefly of a visual and vocal-motor auditory sort, having to do, usually, with past sittings in the experiment. An examination of the introspections quoted from this series (pp. 332ff.) shows that the certainty of a recognition bears an inverse relation to the multiplicity of processes which intervene between the perception of the odor and the reaction thereto. Her second introspection (45) contains an excellent confirmation of our contention that the essence of the consciousness of familiarity consists in the sequence in attention of processes beginning with an *Aufgabe* to recognize, proceeding to a clear perception of some distinguishing characteristic of the stimulus, and passing, after pause and tension, to an appropriate reaction. The odor of Trèfle was clearly recognized first as a perfume in just such a sequence of processes, though it was not yet familiar enough to be known as the particular fragrance called Trèfle.

The fourth introspection of this series (47) is typical of the initial olfactory recognitions of *Fs*. Here, though her attention takes the general course shown in her learning procedure, still much imagery intervenes between perception and reaction; and the result is a lack of certainty even when the correct name for the perfume finally appears.

During the intermediate stages, these associated and irrelevant processes become fewer, and less compelling to her attention (48); and by the time an odor has become exceedingly well known, such processes have disappeared altogether. We have then a rapid, sure recognition, whose essence consists in the movement of the attention, directed by the *Aufgabe*, from unique stimulus quality to appropriate reaction (49).

*Observer W.*

*Visual series:* Visual, vocal-motor and auditory imagery were almost equally important for *W.* In his attempts to master the visual material,—visual imagery being slightly favored. Every detail of each letter was noted in clear visual perception. His visual imagery consisted chiefly in revivals of letters which had formerly been presented, sometimes of the same, sometimes of different types; in retaining in memory the letter last presented (when an entire alphabet was being displayed) until the next appeared; and often in anticipating certain of the letters which were still to be observed. During the progress of these visual activities, *W.* was aware that he was describing certain characteristics of the types in vocal-motor and auditory imagery, frequently repeating, in these same terms, the name of the type which was being presented.

The attention of this observer is characterized throughout by its mobility. In his initial recognitions, with the first noting of the characteristics of the letter presented, *W.* found that his attention had turned with great celerity to a constantly shifting mass of imagery and affective toning, and that multiple activities such as comparing, discarding, accepting, contrasting, and the like, succeeded each other with kaleidoscopic rapidity. All of these activities he described in terms of kinaesthesia and attention. For example, a typical case of the activity of contrasting occurred when a De Vinne 'A' was presented and *W.* found himself attending to a hazy visual image of larger letters from which the stimulus letter seemed to emerge. This, together with a faint vocal-motor 'small' was for him a consciousness that the letter under consideration belonged to a type-face which was smaller than any other shown him during the investigation. *W.*'s awareness of these particular activities of attention,—turning now to this, now to that image, with well-defined strain, often localized in the vocal-motor apparatus,—constituted for him an experience of familiarity with the material (50). This experience was usually, though not always, affectively agreeable. In his subsequent recognitions, *W.* found that his attention was less and less occupied with revived imagery, comparisons, and the like, and that it tended to turn more quickly from the characteristics of his percept to some sort of motor reaction (51). This latter usually consisted in the actual pronouncing of the name of the typeface to which the letter belonged. The moment which immediately precedes this turn-

ing from the percept was characterized by an awareness of tension in the head or upper body, and by a sudden rise in the intensity of the impression, or,—as *W.* frequently phrased it,—by a ‘consciousness of increased attention to the percept, followed by a relaxation and a complete shift of attention away from it, and toward the vocalization of something.’ This ‘something’ proved to be, usually, the name of one of the type-faces, although the type was not always named correctly even when it had been recognized as very familiar. The awareness of the functioning of the elements concerned,—of the increased tension, the relaxation and the attention shift,—constituted the essentials of the recognition (52).

*Auditory series:* In learning to recognize the musical selections, *W.*’s consciousness was rich in sensory components of great clearness and detail. He usually reported visual imagery of an orchestra, or of certain instruments from which the music seemed to proceed; and he was frequently conscious of visual imagery of printed music. Remembrances of other airs, in auditory and vocal-motor terms, frequently appeared, as well as motor imagery of singing the selection which was being presented. Kinaesthesia was exceedingly important; it usually consisted of innervations of the muscles of the hands, arms, face, neck, feet, legs and chest,—their order of importance following the order named. Occasionally, kinaesthesia functioned as the only sensory component in his recognitive consciousness.

In his recognitions of the musical selections, *W.*’s attention exhibited the same general course of activity as that just described. In the earlier recognitions, while the airs were as yet only slightly familiar, it was occupied with groups of imagery and kinaestheses, groping amid multitudinous details of present and recalled experiences until some appropriate motor response ensued. When such reaction finally occurred, *W.*’s attention instantly shifted,—turning from both the stimulus and from his recent reaction thereto (53).

As the airs became more familiar, the number of activities engaging his attention became proportionately less, though here, as always, the general course of his procedure was the same, from the noting of certain characteristics of the stimulus, to some appropriate reaction to it (54).

Finally, when a very familiar selection was played, *W.*’s attention hurried from a strained noting of some striking feature of the air, of the rhythm or of his concomitant vocal-motor auditory imagery, to his usual reaction to the familiar air, complete relaxation ensuing (55).



*Tactual series:* *W.* employed little imagery other than visual in his learning of the New York Point letters. His perception of the number and arrangement of the points was obtained by means of tactual sensations, sometimes supplemented by kinaesthesia in the finger or forearm,—when he actually moved his finger over the points repeatedly and always in the same direction. The perception thus obtained was succeeded by an instant visualization of a group of dots of definite number and arrangement, and, usually, the letter for which they stood appeared beside the dots in the visual image.

In his earlier recognitions of the tactual material, besides rapidly noting and comparing the percept and the associated visual images, *W.*'s attention frequently shifted to a very complete reconstruction of one or more of the letter symbols which had been formerly presented to him (57).

As the letter symbols became more familiar, there was less recall of past situations. However, a clear visualization of the arrangement and number of points, as well as of the letter which these points represented was a necessity throughout the intermediate stage of the mechanization of the act of recognizing these symbols. The introspections show, too, that the decrease in the number of processes making up these intermediate recognitions was in the nature of a telescoping of those originally present, as the general course of events tended to remain constant throughout subsequent experiences with any given letter symbol (58, 59).

It was only in his mechanized recognitions of these materials that no visual imagery of printed letters made its appearance. Here, his attention hurried from the noted characteristic of the tactual stimulus to a pronunciation of its name. *W.* describes this experience as so thoroughly mechanized that he was not conscious of any familiarity with the given stimulus,—the reaction seemed entirely automatic (60).

*Olfactory series:* *W.* took up a rather passive attitude in his learning of the olfactory stimuli. Like *Fs.*, he seemed to find his experiences interesting and abandoned himself to them in a manner which seemed to give them, in marked degree, the spontaneity and naturalness so earnestly striven for in laboratory procedure. During the smelling of the odors to be learned, his consciousness was rich in imagery of many modalities, the visual predominating. His visual imagery was very profuse and varied. Certain images came to be permanently associated with certain odors, as, a clover field with Trèfle, a pot-pourri with Rose. Auditory and vocal-motor imagery of names and characterizations of the odors were

also abundant; and olfactory imagery was present with surprising frequency.

The most unique and characteristic thing connected with *W.*'s learning of these stimuli, however, was his tendency to localize certain odors in certain definite parts of his nose. Each, too, had its own tactual qualities. Thus, Coast Violet 'strikes low in the nostril, and is smooth and oily'; Amyl alcohol appeared to be experienced 'far back, and down in the throat'; Quelques Fleurs is 'a diffused, prickling sensation, high in the nose'; Ethyl alcohol is a 'vapor-like and watery cone which seems to start low in the nose and then to spread out.'

In learning the Coast Violet perfume, recognitions of which are quoted (pp. 336ff.) *W.* invariably perceived the pungency of the sensation in visual and tactual terms, which he described 'as visualizing the lower part of his nasal lining, which seemed to be smooth and oily.' Visual imagery of a meadow, in which plants like skunk-cabbages were growing, was also present whenever this odor was given him.

Familiarity with an odor, in *W.*'s case, was always the result of a definite sort of attention behavior: a movement in the direction invariably followed in learning the material, from *Aufgabe* to characteristic feature, thence to some striking association,—usually of a visual order,—and finally to an appropriate motor response. The latter, in this series, was often non-focal, in which case the attention had withdrawn from the problem and turned to something else, *i. e.*, the introspection, the situation, etc.

In recognitions of the initial sort, the experience was very complex; and associations and activities of many kinds found their way into the stream of attentive processes. In proportion to the interruption and delay thus caused, surety of identification became less, though the experience of familiarity was often unaffected thereby. Let the order of associations be disturbed, whether by the interference of images not formerly a part of the experience with the odor under examination, or by sensations of a new character (64), or by the absence of some important element, and the odor might not be identified at all, or might cease to seem familiar.

The dropping out of the less important elements of the original sequence of processes, did not lessen the intensity of the experience of familiarity. This is exactly what happened in the recognitions which we call intermediate (65).

In the final stages of these recognitions, the attention was still much more occupied with imagery than was the case in

any other of the materials used (66). The essential thing to note in these experiences is the movement of the attention activity from stimulus quality to reaction, the time interval between becoming less as mechanization proceeds.

*Observer V.*

*Auditory series:* During her learning of the musical selections, *V.* was constantly aware of visual imagery of unusual detail, color and variety, as well as of much emphatic inner-variation and imagery. The visual imagery included detailed representations of orchestras, of particular instruments, of stages set for opera, and the like. In many instances, the clang-tint of a particular instrument was represented in her consciousness by symbolic imagery sometimes accompanied by empathy. For example, the clang-tint of a bass horn in the Tschaikowsky Symphony was represented in her consciousness as a large, formless, dark, smooth mass; and accompanying this imagery was a unique experience which she described as a feeling that her own body was being expanded and diffused.

The temporal sequence of the events which occupied the focus of *V.*'s attention, together with their nature and duration, was a very important factor in each of the several stages of her consciousness of familiarity with this auditory material. In the initial stage, she was first aware of attentively noting the stimulus,—the clearness of her perception of it constituting one factor in her consciousness of its familiarity. Then her attention was attracted to kinaesthesia and organic sensations; these almost invariably consisted in internal imitations of the rhythm, and constituted her perception of that rhythm. If no name for the selection yet appeared and claimed her attention, the latter was occupied with a renewed noting of the stimulus. This stage was frequently characterized by the presence of associated imagery, which was dominantly visual (62). Some appropriate motor reaction ultimately ensued,—its implicit suitability consisting merely in the fact that on its completion her attention withdrew completely from the problem and turned to an introspection of the processes which she had just experienced.

As her recognitions became more rapid and sure, *V.*'s attention was occupied with fewer remembrances of her former experiences with the selections, though striking visual images associated with an air tended to persist and to occur in the same sequence of events throughout (68). Her attention

came more and more to shift from an awareness of a satisfactory adjustment to it, followed by organic sensations of relaxation (69, 70). The latter form of procedure is characteristic of the final form of her experience of recognition.

*Tactual series:* During her learning of the point letters, *V.* was actively engaged in obtaining tactual and kinaesthetic impressions of each stimulus presented, and in instantly visualizing each in turn.

*V.*'s experiences with the tactual stimuli were characterized by an attention behavior similar to that described in her auditory recognitions. In her initial experiences, while the stimuli had as yet no high degree of familiarity, she reported a keenly active attention, occupied with obtaining a clearer perception of the stimulus presented, and with a visualization of certain characteristics of that perception. Complete certainty, however, was not attained until a name for the stimulus was pronounced. In the intermediate stages of mechanization, her act of recognizing was characterized by a somewhat less active attention, engaged with fewer processes; a name for the stimulus here became focal more rapidly. Finally, when the touch symbols had become thoroughly familiar, *V.*'s attention leaped from her tactual perception, with its accompanying tension, to an almost instantaneous kinaesthetic relaxation,—this sequence of attentive processes constituting her consciousness of familiarity with the stimulus. The subsequent appearance of a visual image of the letter whose symbol had been presented was entirely incidental to the act of recognizing (73).

*Olfactory series:* *V.*'s learning of the odors was characterized by an eager, interested attitude. She remembers odors readily, and they are, for her, usually rich in associations. The affective toning possessed by certain of these stimuli proved to be a great aid in her later identification of them. Her procedure in learning the perfume called *Quelques Fleurs* is typical and I can do no better than to quote her description of it:

"In the fore-period (*i. e.*, between the pronunciation of the perfume's name and its presentation) I was visualizing the name written in Japanese letters, and pronouncing it to myself. I also had a visual image of 'Some Flowers,' printed in bold type. When I got the odor I experienced, first, a marked degree of pleasure. Then my attention turned to a visual image of a bunch of wild flowers, violet and pink in color, with pale green leaves,—held tightly in a small hand.

I saw the tiny fingers but no thumb, and was aware of smiling with great pleasure. Then I attended to the stimulus again, but the odor seemed to take me to a forest path, with big, cool tree trunks, and large sweeps of green. It was all very pleasant. I ended by saying 'Quelques Fleurs' several times."

In all her recognitions of the odors, *V.* attended chiefly to the affective toning which each of the different stimuli aroused. In the earlier experiences with a given stimulus, her attention was next attracted to a succession of images such as had constituted her experience in learning the particular odor under consideration. These images, chiefly visual, often underwent considerable condensation and displacement. For example, color, which was associated with a bouquet in learning, sometimes appeared in a recognition merely as tinting the letters of a verbal image of the perfume's name (74). In intermediate stages, such associated imagery frequently occupied her attention for only the briefest instant, and then often in mutilated, abbreviated or embellished form. The essential thing here, as in every level of familiarity, was that the attention followed a given procedure,—passing from percept through emotional toning to appropriate reaction, under the direction of a recognition *Aufgabe*. In the final stage, no processes intervened between these salient features of the experience (77).

*Observer B.*<sup>16</sup>

*Auditory series:* *B.*'s attention, in his earlier recognitions of the musical selections, was, like *W.*'s, first occupied in noting various characteristics of the stimulus and in comparing them with imagery of his former experiences with the same stimuli. Motor and organic phenomena occupied a prominent place in his attention activity, as also did various visual schemata (81). Kinaesthetic experiences of swaying, beating time, and the like, were frequently focal in his attention; and when present they invariably constituted an important factor in *B.*'s consciousness of familiarity (78). In his later recognitions, *B.*'s attention was less frequently occupied with remembrances of his former experiences with the musical selections. His recognitions during the intermediate stage were almost invariably characterized by the fact that kinaesthetic and organic sensations occupied the focus of attention; and these sensations were usually accompanied by anticipatory images of the air, in auditory terms (79).

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<sup>16</sup> *B.* served only on the auditory series.

In his final experience, however, *B.*'s attention first seized upon the clearness and facility of his perception, and then turned to his customary response (80).

*Observer O.*<sup>17</sup>

*Olfactory series:* *O.* doubted his ability to learn the odors used in the olfactory series, and both his overt behavior and his introspections testify to a certain strain and anxiety in his attitude. He succeeded in mastering only a few of them. Vocal-motor imagery and affective toning proved to be his best aids to identification, and he attended to them, almost exclusively, during both learning and recognizing experiences.

The course of *O.*'s attention at different levels in the mechanizing of his recognitions of a given odor needs no special comment. An examination of a typical series of his introspections (pp. 34of.) reveals a procedure like that already described in the cases of all the observers. His early recognitions are less rich in associated imagery than are those of many of our observers, but so also are his learning experiences.

*Observer S.*

*Olfactory series:* *S.*'s attitude in these experiments on smell recognitions was that of one intent on an interesting and pleasant activity. He is convinced that olfactory images are common in his experience and even make up a large part of his mental life. He remembers odors readily, and finds them very potent in arousing associations.

His learning of the odors consisted in associating their names with the particular sensations and feelings which they aroused; and this process was invariably attended by detailed visual and vocal-motor imagery. Cresol, for example, was marked in his consciousness by the fact that the odor seemed always to affect a particular part of his nose, in a particular way, all of which he visualized. He noted, also, that its smell quality persisted longer than was usual with the other odors of the series; and this experience of duration seems to have been a very definite conscious datum,—an attribute of the sensation in question (85). On its first presentation, he experienced detailed imagery of a bottle of Mapleine, and of the odor of maple sugar.<sup>18</sup> Revivals of these images, in various degrees of integrity, persisted into the later stages of his recognitions of Cresol.

<sup>17</sup> Observers *O.* and *S.* and *Bd.* served only in the olfactory series.

<sup>18</sup> See footnote, p. 341.

In the initial experiences, S.'s attention was engaged not only with the location and persistent quality of the sensation, but also with detailed revivals of visual and other images which had been associated with the Cresol odor during the first learning presentation (85). As the odor became more and more familiar, this imagery became telescoped, abbreviated or lessened in amount; when the odor had become exceedingly familiar, no imagery remained. S. found his attention first occupied with the persistence of the peculiar tactual and olfactory qualities of the sensation, and noted the passing, with slight pause, to a motor response. In the particular case quoted, this response consisted in speaking the odor's name. A motor response of a different kind and one often described by S. as largely constituting his awareness that the stimulus was a familiar one, consists merely in a sudden inhibition of his inhalation of the odor.

*Observer Bd.*

*Olfactory series:* Bd. approached the problem of learning to identify odors with considerable misgiving. He was unaware, when the experiment began, that he ever experienced olfactory imagery; and he was of the opinion that he would be slow in learning the odors presented. He took up a very active attitude during each sitting,—an attitude which was dominated by his keen awareness of the task. Here is his own description of his typical learning procedure:

"I am very clearly aware of an *Aufgabe* to remember definite characteristics of each odor. This is present in vocal-motor terms; I sometimes feel myself tending to say 'very definite characteristic,' but in very much abbreviated form."

In learning the perfume called Halcyon Rose, recognitions of which are described in the introspections quoted (pp. 342f.), he says:

"I was aware of very keen concentration of attention upon the task in hand, and of the *Aufgabe* to identify the odor, which was present in the word 'identify' appearing, with a rising inflection, in vocal-motor imagery. I noted the characteristic of the odor, that briar-rose quality, and tended to repeat 'briar' and 'rose' during the sniffing."

In his initial stage of familiarity with any of the odors learned in this series, Bd. described partial or complete repetitions of his former experiences with the odor in question. He found his attention moving from event to event, in the

sequence which had formerly obtained. Moreover, he reported not only awareness of the succession of sensory and affective components, but also a consciousness of the nature of his attentive activity. 'I was aware of very keen concentration' is a very common phrase in his introspections; so also are descriptions of 'actively waiting,'—a state which he believed was something over and above the muscular tension involved in both.

The intermediate and mechanized stages of *Bd.*'s familiarity with this material was marked by a decreasing temporal duration, a lessening of the strain and of the searching activities. In the final stages, especially, the sense of familiarity welled up immediately (95, 96) upon his perception of the odor quality,—his attention turning almost instantly to a motor response. He again and again described his 'feeling of familiarity' as a 'complex of organic sensation and kinaesthesia, pleasantly toned, not localized at any particular point, but chiefly about the chest and upward.' The introspective evidence shows that such states are always preceded by a consciousness of the recognition-task together with a perception of sensation quality, and that they are succeeded rapidly by relaxation and shift of attention.

*β. Conclusions.* A survey of the introspective evidence, and of the foregoing discussion of the conscious processes which they record, leads to the following conclusions.

The mental behavior peculiar to the act of recognizing is invariably induced by a certain attitude which may be called a 'preparedness to recognize.' This attitude is the immediate result of the task set the observer. Under our experimental conditions, he was instructed to attend to the stimulus, and to decide whether it was known or unknown to him. After the first sitting, the act of merely placing himself before the apparatus was sufficient to induce a recognitive attitude. The observer was often keenly aware of this preparedness, especially in the period immediately following the 'ready' signal; and many descriptions of its structural content were given. It frequently took the forms of vocal-motor processes such as, 'What is it?' or, oftener still, a telescoped form of the first word only,—'Wha?' Consciousness of diffused bodily strains; awareness of the hand poised above the reaction-key; tension in the vocal-motor organs, as though about to pronounce a name; visual imagery of the experimenter's face wearing an expectant look,—these and many other processes were again and again reported as components of the consciousness of the task,—or as its entire and adequate sensory content.



When a known stimulus was presented, the preparedness to recognize, explicit or implicit in the activity itself, induced a behavior of attention which was characteristic of the cognitive consciousness. When the stimulus was presented, the attention immediately fastened upon distinctive features, or upon a distinctive feature, which had been noted previously. If the stimulus was still relatively unfamiliar, the attention was next occupied with revived associations; and it fluctuated between these and farther notings of characteristics of the stimulus, until its name appeared in consciousness, or until an appropriate reaction was made. The duration of the experiences constantly decreases.

As the stimuli grew more familiar, certain of these associative activities and components dropped out, until finally, not even the name remained necessary for complete recognition,—the appropriate reaction following immediately upon the clear perception of certain characteristics of the stimulus. Three components of the cognitive experience, however, were invariably present in orderly sequence: the preparedness to recognize; the clear perception (more or less prolonged) of some characteristic feature of the stimulus; an appropriate reaction. The absence of any one of these components, or a change in their order of appearance resulted, invariably, in an absence of the experience of familiarity. This particular behavior comprises a sequential, synthesized experience which is the consciousness of familiarity.

### γ. Summary

1. The process of recognizing, whose essence is an awareness of familiarity, exhibited many degrees of complexity, among which at least three well-marked stages or levels were distinguished.<sup>19</sup>

2. Each of these three levels, which may be called initial, intermediate and final, was characterized by a mode of attention behavior which, though showing certain features common to all the stages, was nevertheless unique in certain particulars.

3. At all levels of familiarity, the attention was under the dominance of a recognition-*Aufgabe*,—a task 'to recognize.' This *Aufgabe* was often consciously present; sometimes it was implicitly present in the mode of the activities which devolved and in the reaction which completed each experience.

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<sup>19</sup> We do not include here the level of complete mechanization, where reactions to stimuli are automatic and instantaneous, and where no awareness of familiarity is present.

4. At all levels of familiarity the attention moved, under this domination of *Aufgabe*, in a given sequence from clearly perceived stimulus through appropriate reaction to relaxation and shift of focus. This is, also, a complete description of its behavior in the final stage of familiarity.

5. In the initial stage, the attention was characterized by the fact that it dwelt longer on the stimulus quality or qualities perceived, and in that it was next claimed by acts of comparing, contrasting, rejecting and the like, having to do with revived imagery and present percept. These acts were interpolated, in the sequence described above, between the initial perception of the stimulus and the appropriate reaction.

6. In the intermediate stage, which itself often included varying numbers of levels, each marked by differences in duration and complexity, the attention to the stimulus and to processes subsequently aroused was of shorter duration. Furthermore, the processes which claimed attention became progressively fewer and more syncopated.

7. The awareness, or 'feeling' of familiarity is not a moment of consciousness, immediate and static; it is a progressing consciousness, made up of definite sequences, and including an awareness of the temporal duration of the processes preceding the motor reaction appropriate to the stimulus.

8. The actual magnitude of this temporal duration bears no constant relation to the intensity of the experience of familiarity, which requires merely that a *perceptible* time interval shall intervene between perception and reaction. When no such duration attribute is present to consciousness, an appropriate reaction to the stimulus may take place without the arousal of any "feeling of familiarity."

9. The degree of familiarity, as measured by ease of identification, bears an inverse relation to the time consumed in recognition.

### C. *Structural Components of Recognition.*

a. *Sensory components.* It has already been stated that this study has demonstrated the existence of different stages or levels of familiarity, each of which is characterized by certain functional traits. Structural differences are also apparent. Obviously, since recognition is an awareness built up through successive experiences, there are no constant or sharply defined lines of separation between the stages here named and described; yet their characteristics are distinctive and have, it would seem, an important bearing on the traditional controversy as to whether or not recognition is an imageless

process. If our results have universal validity, the act of recognizing may be imageless, but may not be devoid of both imagery and sensation; and the more mechanized the process becomes, the nearer it approaches a non-sensory content. Those investigators who find that imagery is always present in recognitions would seem to have examined recognitions only in the initial or in some intermediate stage of mechanization. Such recognitions, obviously, are exceedingly numerous and important; but they by no means exhaust the possibilities of recognitive experience.

In our discussion of individual cases, under the heading of functional components of recognition, a somewhat detailed account was necessarily given of the structural components which were present in the recognitive experiences of each observer at different levels of his increasing familiarization with the stimuli employed. We refer the reader to these analyses and to the typical introspections quoted from the protocol of each observer, for confirmation of the following conclusions regarding the sensory components of the process of recognizing.

When stimuli having only a slight degree of familiarity for an observer were presented to him, his consciousness was rich in sensory components. He was aware not only of the sensations aroused by various aspects of the stimulus itself, but of a varied mass of images and sensations, sometimes affectively toned, and usually having a direct connection with his former experiences with the stimuli in question. The sort of imagery which was most abundant and detailed in such experiences seemed to be that favored in the imaginal type of the observer, rather than that of the sense modality to which the stimulus belonged.

We offer this latter point merely as a suggestive probability, —for no thoroughgoing tests to determine the imaginal types of our observers were undertaken. Such tests would have been a problem in themselves, as Fernald<sup>20</sup> has amply demonstrated. The imaginal type of each of our observers was roughly diagnosed from data at hand, obtained in numerous experiments in the Clark laboratory, where all had been working for periods ranging from one to four years. According to this classification, *Fs.*, *B.*, and *S.* were of the balanced type, using imagery of several modalities with equal facility; *W.*,

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<sup>20</sup> Fernald, M. R., "The Diagnosis of Mental Imagery," *Psychol. Monog.*, 1912, XIV, I., p. 169.

*V.*, and *Fn.* were of the mixed type, with marked preference for visual imagery; *Bd.*, *F.*, and *O.* were of the mixed type, with preference for vocal-motor imagery. *F.*'s vocal-motor preference is especially marked.

As the experimental materials became more and more familiar in successive presentations, there was a signal decrease in the amount of imagery present in the recognitive experiences. Its details, also, tended to be lessened and there were frequent examples of noticeable decay or alteration in the revived images which appeared. In some cases interesting condensations occurred, details of several images being fused into one. Organic sensations and kinaesthesia became more prominent in these mechanizing stages and the time required for each recognition grew notably shorter. Finally, when the materials were so well learned that their recognition was certain and immediate,<sup>21</sup> sensory content, though still present, was usually reduced to its lowest terms: an appropriate bodily reaction, often merely organic and kinaesthetic tension and relaxation.

*b. Affective toning.* It has long been maintained that pleasant affective toning is an indispensable component of the process of recognizing. A formidable list of psychologists supports this view.<sup>22</sup> James likens this pleasurable feeling to the warmth experienced on meeting an old friend. In the case of meeting, in a dark alley, a well-known enemy, this position would surely have to be defended by assuming that the glow of pleasure which is necessarily a part of the recognition is still present, but is quite swamped in other feelings aroused by this known object. In other words, the pleasure was there, but could not be felt! Happily, experimental evidence makes such logical assumption unnecessary. Affective toning appeared in less than forty-five per cent of the fifteen hundred recognitive processes analysed during these experiments; and in three per cent of these cases, the feeling was unpleasant.

Pleasantness often appeared as part of the consciousness of having successfully performed the task assigned. It also accompanied many sensations aroused by the stimuli, whether or not they were known. This was especially true in the auditory series, and in the cases of certain of the perfumes

<sup>21</sup> The term 'immediate' is used in its relation to the time required, and not in the sense of 'underived.'

<sup>22</sup> Cf. among others: Wundt, 58, 536; Külpe, 56, 172; Titchener, 57, 408; James, 54, 650; Meumann, 21, 40; Katzaroff, 55, 75.

used in the olfactory experiments.<sup>23</sup> Pleasantness was entirely lacking, however, in a great proportion of the recognitions described by our observers. It was, therefore, not a necessary component of these recognitive processes; and we believe an examination of the experiences of everyday life will justify extension of this contention to recognitions in general. The recognition of a pleasing object is a pleasantly toned experience; the recognition of a disagreeable object is often an experience essentially unpleasant. There is one case in which the latter does not hold, the case where the ability to identify the object chances to be of prime importance. In such a case the consciousness may be dominantly pleasant; but, even here, the affective toning need not be a component of the recognition but rather it may color the mental state immediately succeeding that recognition,—a state of awareness of having correctly performed a given task.

The experimental results show that affective toning is absent in so large a number of cases, in each of the levels of familiarization, that it seems necessary to assume that feeling-tone bears no essential relation to recognition, in any of its stages.

c. *Consciousness of self.* The doctrine that consciousness of self is always present in recognition has long appeared in the writings of psychologists.<sup>24</sup> The term 'consciousness of self' is an ambiguous one; and there is apparently no agreement as to the exact nature of such a consciousness. Titchener (57, 544) describes the consciousness of self as 'the particular combination of talent, temperament and character,—the intellectual, emotive and active constitution,—that makes up the individual mind.' The self-experience has "certain fairly constant constituents: organic sensations, a visual perception or idea of the body, and verbal ideas of 'I' and 'my.'"

Calkins, who believes that consciousness of self constitutes the background of all mental experiences, conceives the 'self' consciousness as a reality "immediately experienced as possessed of at least four fundamental characters" (46, 3). These characters are: relative persistence, complexity, uniqueness and relatedness.

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<sup>23</sup> In some special cases, the affective toning aroused by the stimulus was attended to and made a criterion of identification. (Cf. observer V., pp. 364 f.) This is, of course, in no way incompatible with our contention that affective toning is not an essential component of the act of recognizing, for in such cases pleasantness was as much a characteristic feature of the experience examined as was the form of a Della Robbia letter, or the rhythm of a melody.

<sup>24</sup> Among others: James Mill, 62, I. 329-339; Claparède, 59, 78-89; Katzaroff, 55, 78; Calkins, 46, 130.

If one adopts Titchener's description of the consciousness of self, there is certainly no warrant to be found in the results of our experiments, for making such a consciousness an essential component of recognition. A study of all the recognitions reported in our experiments yields a remarkably small percentage of cases in which the observer was aware of self, during the recognitive act. In the entire visual series, for example, *W.* was aware of 'self' in five out of seventy-three recognitions; *Fn.* described two such cases in seventy experiences; *Fs.* one out of eighty; and *F.* none at all in his fifty cases. A similar condition is apparent in the introspections on the other kinds of experimental material used.

If, on the other hand, Calkins' conception of the 'self' experience is adopted, the evidence is not quite so unequivocal. *Fn.*, for example, stated plainly that he believes that he is 'always more or less dimly aware of himself as the acting subject' in all his mental experiences. If this be the case, however, the consciousness of self is not a distinctive mark of his recognitions *per se*, but a component of all his mental activity. Moreover, the other eight observers not only did not find the 'self' a constant component, but were even convinced that it was relatively rare in their recognitive experiences.

d. *Summary.* An analysis of the data yielded by some fifteen hundred introspections upon recognition justifies a number of conclusions concerning the sensory components of the act of recognizing:

1. Sensory elements are invariably present. Imageless recognitions are not unusual in the experience of our observers; indeed, reproduced imagery is the exception rather than the rule in the recognizing of very familiar stimuli. But sensationless recognitions,—recognitive processes devoid of kinæsthesia or organic sensations, were never experienced by our observers.

2. Three well-marked stages in which the sensory content differs not only in quantity and duration but often in quality as well, make their appearance in the introspective accounts of each of our observers during his progressive familiarization with a given stimulus. These stages may be called the initial, the intermediate and the final stages.

3. In the initial stage, where recognition of slightly familiar objects takes place, the sensory content is rich and varied. The sort of imagery chiefly employed, seems to be that favored in the imaginal type of the observer, no matter what the

modality of the stimulus recognized, i. e., whether it be visual, auditory, tactual or olfactory.

4. The intermediate stage is characterized by a sensory content which is less full and less varied,—the imagery of the initial stage tending to telescope, to lose detail, to alter in various particulars, or even to disappear.<sup>25</sup>

5. In the final or mechanized stage the sensory content is exceedingly attenuated. Imagery is often entirely lacking, though motor or organic constituents never fail to appear. When imagery is present, it tends to be of the modality of the stimulus recognized in a greater percentage of cases than is found in the initial recognitions.

6. Affective toning is not an essential component of the act of recognizing, at any of its stages.

7. Awareness of 'self' is not a necessary component of recognition.

#### *D. Non-recognition*

*a. Introspections and interpretations.* Descriptions of experiences where stimuli which had formerly been presented were believed to be new, shed much light on the problem of recognition. Many cases of this sort were described by our observers. A few typical cases, selected from different series of the experiment, will be quoted and briefly discussed.

After his first presentation of the Bulfinch type of learning, the letter 'B' was presented to Fn. for recognition.

Fn., Bulfinch 'B,' 3.6 sec. "New. I never saw that letter. I did a great deal of thinking here. My perception of it was very clear, but I just attended to a little part of it at a time. My visual schema of five places came up, but the places in it did not seem stable; it seemed to make no difference whether they were at one spot or another. I finally tried to call up images of all the 'B's' and place each in its proper place in the schema. I found I could not get clear images of them and I gave it up. It seems absolutely new."

The schema of which Fn. here speaks has already been described (p. 351); and a reference to his descriptions of recognitions of Bulfinch will show the prominent part which it played in his familiarity with this type. In the experience quoted above, Fn.'s attention did not behave as it usually did in his encounters with known letters. First of all, instead of being directed to a characteristic feature or group of features,

<sup>25</sup> This intermediate stage may manifest numerous levels. The number and the constituents of these levels seem to depend upon a multiplicity of factors, such as: individual differences, complexity of material, length of interval between exposures, etc.

he found that his attention wandered over the letter, examining it bit by bit. Next, on the appearance of his schema, instead of attending to a particular part of it as was the case when he experienced familiarity with the stimulus, he found it all confused and apparently equally unimportant in all its parts. Finally, instead of passing from percept to habitual and appropriate reactions, he was aware of much interpolated mental activity and continued strain.

One of *V.*'s experiences, in which she failed to recognize a familiar odor, is instructive. In learning the perfume called Coast Violet, she had immediately associated the odor and its name with clear visual imagery of pale violets growing on flat sand dunes,—an image directly traceable to experiences at her favorite coast resort. This was accompanied by very pleasant affective toning, and supplemented by a frequent repetition of the words 'Coast Violet' in vocal-motor imagery. Her subsequent recognitions of this odor made use of the same experiences; their number decreased rapidly, but no new activities or processes were present. Two weeks after her first learning of the odor, though she had recognized it as familiar in the meantime, she reported the following experience.

Observer *V.*, Coast Violet, 21 sec. "I think I've never smelled this before. I tried, excitedly, to place it in a former sitting,—this in a strong contraction of muscles as though to face in the direction of the room in which we sat last time. This faded out of consciousness and I leaned forward in an attempt to get more of the peculiar quality of the odor. Pleasant affective toning welled up, and vocal-motor imagery 'perfume.' Then a visual image of a bright bed of flowers,—tulips, I think. Then my attention seemed entirely taken up with an auditory image of your voice saying 'introspect.' There was no familiarity in the whole process."

Here, from the very inception of the experience, a new set of images and activities ran their course, and no 'feeling' of familiarity resulted, or, as I should prefer to put it, this experience of a new sequence of mental events *was* the experience of unfamiliarity.

*Bd.*'s introspections in the olfactory experiment furnish several cases of absolute non-recognition of odors which had been presented before, or even partially learned previously.

Observer *Bd.*, Coast Violet, 10 sec. "No familiarity. While I was smelling it, two or three names occurred in vocal-motor terms, but in very fleeting form: 'Violet, Rose.' They slipped right through consciousness: there was no tendency to accept one rather than another,—indeed, none seemed appropriate."



In cases where *Bd.* recognized this odor, his experience followed the same sequence which occurred in his learning experiences, *i. e.*, awareness of odor quality, organic sensation of relaxation, together with the name 'Coast Violet' in vocal-motor terms, and immediate shift of attention.

A typical description of *B.*'s recognitions of the Rigoletto selection has already been quoted (p. 340). The following is a case where this air seemed quite new and unfamiliar up to a certain point in the experience,—namely, the point at which the sequence of processes formerly experienced with this air enters consciousness.

Observer *B.*, Rigoletto, 15 sec. "It was some time before I realized that that was a familiar air. I kept trying to place it in the experiment and make it seem familiar,—this in terms of calling up former sittings in this room, imagery of you, of myself and of some auditory snatches of airs. But the whole thing was confused. I felt tense and puzzled. Then suddenly I fell into the rhythm and the air began immediately to seem familiar."

*b. Confirmatory evidence.* To our own cases, already quoted, may be added an exceedingly interesting case recently published by Borel (43). A French medical student of his acquaintance described to Borel two experiences of '*jamais vu*,' each of which occurred as he was walking down a street which he had traversed many times. On these two occasions, however, he was walking in a direction opposite to that usually taken. He experienced, each time, a feeling of utter unfamiliarity, though in neither case was there any strain, anxiety or unpleasantness. Borel believes that the phenomena were due to the reversal of the order in which the perceptions were experienced. He inclines to Bergson's view of recognition as an awareness of an adopted motor response (42, p. III) and maintains that it is difficult to change the order of such responses. Therefore, he concludes, when series of perceptions are aroused in their usual order, a feeling of recognition results, but if perceptions are aroused in an unusual order, non-recognition ensues.

But non-recognition can not be dependent entirely on the order in which perceptions come to consciousness, else every one walking down a familiar street in reverse direction would repeat the experience of the French medical student. Moreover, in our own cases of non-recognition the perceptions often arrived in normal sequence, but the subsequent mental processes did not. Borel's idea of the necessity of the accustomed sequence of mental activities is in agreement with our own; but we would not confine those activities to the perceptions of the objects recognized. The sequence which our data seem

to us to establish as essential to recognition proceeds, in the most nearly mechanized cases which still have the awareness of familiarity, from *Aufgabe* to clear perception, thence to appropriate reaction, culminating in a shift of attention. Intermediate processes of associating, comparing, and the like, occur in the case of less familiar material.

This envisagement of recognition as an awareness of such a sequence of events is well suited to explain the experience of unfamiliarity reported by Severance and Washburn (69) where the observers' attention was concentrated upon a given object until its familiarity vanished. The normal sequence of mental events was not permitted to run its course.

### *E. False Recognitions.*

During the course of these experiments, there occurred, with each observer, several cases in which 'new' stimuli were falsely recognized as 'old.' We shall quote a number of typical cases and discuss what seems to be the significant features of these paramnesias.

Observer *Fs.*, *Quelques Fleurs*. (New), 2.4 sec. "That's violet. Almost instantly I was aware that that was a fragrant odor. My attention singled out the unmistakable violet character. The vocal-motor image 'vi' occurred and then I withdrew the bottle and said 'That's violet.'"

*Fs.* gave every evidence here of being certain of her identification. The odor seemed entirely familiar, though it was, as a matter of fact, quite new to her. Soon afterwards she learned to recognize this odor easily and unfailingly; and on a later occasion she stated that the odor was not at all like violet in quality and unlike any other she had ever smelled. Her whole procedure, however, in the experience quoted above, was identical with that characterizing her usual experience with the violet odor. It consisted in a preparedness to recognize (evinced in a tense attitude and an expectant expression) and a clear presence in consciousness of a perfume quality, followed by the uninhibited vocal-motor image of a known perfume's name, which was then actually spoken aloud with conviction.

Observer *F.*, *Hansel and Gretel*. (New), 20 sec. "At first I thought it was going to be familiar. I began humming along with it and 'Meyerbeer's Prophet' came to mind in auditory imagery. But instead of accepting this, I found myself repeating it with a questioning accent. My humming had ceased, my tension increased, and all the familiarity was gone."

Here again, we find the experience of familiarity to be an habitual behavior of attention. *F.* finds that he has been

experiencing all the events which usually compose his total reaction to a known air, and the air seems familiar. But when, instead of experiencing the usual reaction and then turning from the experience to attend to something else, he finds his attention lingering on it, repeating part of it, hesitatingly, then all sense of familiarity departs.

*Fs.* had a similar experience on hearing, for the first time, the selection from Humperdinck's opera.

Observer *Fs.*, Hansel and Gretel. (New), 30 sec. "I was sure that was a familiar one at first. I really recognized it as Loch Lomond. The experience was like this. I was prepared for an old air,—this in terms of adaptation for listening, with my hand poised above the reaction-key. When the music began, I started to hum. My vocal apparatus formed itself to say 'Loch' and I felt my finger innervated to give the tap. But the movement was inhibited, apparently through the shift of my attention to the characteristics of the music again. The air grew very clear and insistent; I ceased humming and became very tense. Now I was very actively noting the music and experiencing recalls of other bits you have played to me here. No name for this came up, and I finally gave you the signal-tap. I'm still keyed up over that. What in the world is it?"

In the first part of this experience is a typical example of false recognition. The auditory stimulus, flooding into a consciousness dominated by a recognition-*Aufgabe*, was at first followed by the course of mental events characteristic of the experience of familiarity: the singling out of a feature to be noted; bodily adjustment to the stimulus, which took the form of voiceless humming; partially imaged pronunciation of a name; and an initiated reaction on the signal key. The sequence was identical with that present in correct recognition.

The experiments with the New York Point alphabet furnished several instances of false recognition. *W.* describes an experience in which 'i,' which he had never before examined, was recognized as 'a,' which was very familiar to him.

Observer *W.*, New York Point 'i.' (New), 5 sec. "As soon as I perceived that there were two dots, the tactual impression became non-focal, and I attended only to a very clear visual image of the dots; their vertical position seemed very prominent. With the appearance of that visual image, there was a very definite feeling of familiarity; no memory images accompanied it,—the familiarity seemed rather to be in my clear perception of the symbol itself. I took my fingers off the dots and said, 'That's "a."'"

*W.*'s criterion for recognizing the 'a' symbol (which consists of two horizontal burrs), was a visual image of two dots. He always reported that he first had an awareness that there were *two* dots, and then a consciousness of the direction in

which they lay. On being presented for the first time with the 'i' symbol, which consists of two burrs arranged one above the other, his clear visual image of the two dots, an awareness of direction succeeded by the usual reaction to these cues, constituted entire familiarity which culminated in the pronouncing of the name 'a.' Had the awareness of the particular direction been prominent enough in consciousness to inhibit the usual reaction to the 'a' stimulus, the entire color of the consciousness would doubtless have been changed, and uncertainty, if not an actual experience of unfamiliarity would have resulted. Cases of non-recognition of old stimuli present pictures of just such occurrences (pp. 375ff.).

Examples could be multiplied. The significance of these cases of paramnesia is that the mental experience involved in each experience is one which moves in a sequence identical with that present in cases of correct recognition: the same significant detail or details are noted; the same associations (if any) are aroused; the same bodily reaction follows after a perceptible time; the same shift of attention to other matters completes the experience.

Such an interpretation of the phenomena of '*déjà vu*' would seem to explain it better than does that of Lalande (67) who assumes that cases of this sort occur with stimuli of which there really has been a double perception, the first being unconscious; when the two become integrated a feeling of familiarity subsequently arises. This account of 'false recognition' really explains it out of existence. There are doubtless many cases of apparently false recognition which could be accounted for in some such way. But many others do not seem to admit of such an explanation. The cases quoted from our own experiments can not be so explained, for our experimental conditions and our introspective evidence rule out the possibility of such a double perception.

Bourdon (15) points out that it is exceedingly difficult to find a case of a wholly new experience in the consciousness of the adult; and he holds that in all cases of '*déjà vu*,' the stimulus which is recognized resembles one which was formerly experienced. He explains this by invoking a 'feeling of familiarity' (due to facilitation of perception) which attaches to the recognized object. But we may accept Bourdon's hypothesis without agreeing with his explanation. Even if all stimuli which seem familiar do resemble others which have belonged to the observer's experience, the important fact remains, that the mental events attending the recognition of these stimuli form a sequence identical with that which char-

acterizes the cognitive experience for stimuli of that modality.

### 3. Conclusion

Our investigation has shown that the experience of familiarity is essentially a motile, flowing consciousness,—the product of a peculiar sequence of clearnesses, durations and adjustments. The apparently simple experience of recognition proves to be a complex process whose degree of complexity varies with the degree of familiarity of the stimulus.

Various levels of cognitive experience may be differentiated: An 'initial' level where the stimulus is but slightly familiar and where the (incipient) recognition is a peculiarly haunting and persistent experience. This level is characterized by a wealth of sensory components and by a keenly active attention, attention usually being busied with searching for a name or for other associations which 'fit' the situation; the consciousness of 'fitness' is essentially a behavior of attention,—when the 'fitting' association appears the search is abandoned and attention shifts elsewhere. The 'intermediate' level differs from the initial level in that it is briefer and much less rich in sensory content. In the 'final' stage the process is very much syncopated; imagery is often wholly lacking here, an appropriate reaction frequently ensuing upon the perception of the stimulus.

Imagery plays a progressively less important rôle during the life history of a complete familiarization; affective processes and the consciousness of self appear so irregularly that they are to be regarded as entirely fortuitous.

There is a functional component, however, which is constant throughout the various levels; and one is impelled to conclude that it constitutes the essence of the cognitive experience. This functional complex consists in an on-going consciousness of activities, of pauses, of adjustments and shifts of attention. In other words, recognition is a sequential experience which, although highly unified for consciousness, may be analyzed into components; and its *sine qua non* is an orderly procession of mental events,—from clear percept through appropriate reaction to attention-shift,—under the direction of the *Aufgabe* or the *Einstellung* to recognize.

## IV. APPENDIX

### A. A Brief Summary of Theories

The theories which have been advanced to explain the nature of the process of recognition fall into two groups, which may be called

theories of underived and theories of derived awareness. In the bibliography which is appended, representative writers on recognition are classified according to the viewpoint which the present author understands them to champion. Theories of the former type describe the consciousness of 'known,'—one's awareness that a given experience is not new,—as a mental datum which is immediately given; this datum is conceived to be a quality which is an inherent part of the perception of a repeated stimulus, irreducible and incapable of further description. Within this type of theory one finds two sub-types,—the 'intellectual' and the 'affective.' The exponents of the former view envisage the consciousness of 'known' as a purely intellectual, non-emotional datum, while for the exponents of the latter view it is dominantly or exclusively affective.

An interesting theory of the underived type is that championed by Washburn (5), who considers the feeling of familiarity to be the psychical accompaniment of the weak excitation of groups of cerebral neurones, whose activity was associated with that of the group excited by the familiar stimulus when it was formerly experienced. If these associated groups are strongly excited, so that images are aroused, she believes that perception and not recognition ensues. But is the recognized object then not perceived? And must an experience of familiarity precede every perception? The fact that the arousal of many associated images does not destroy but often actually intensifies the awareness of familiarity tells against this theory.

The advocates of the affective theory of 'derived awareness' maintain that recognition is an experience which is primarily emotional, a genuine 'feeling' of familiarity which attaches to the perception of known objects. Introspectionists maintain that an appeal to the known results of psychological analyses made in numerous studies suffices to overthrow all *sui generis* theories of recognition,—the 'feeling' of familiarity is analyzable.

The second group embraces those theories which conceive the consciousness of familiarity to be derived from certain elementary processes, sensory or affective, or both. These theories, like those of the *sui generis* or 'underived' group, may be divided into two sub-groups according to the emphasis placed by their sponsors upon the intellectual or upon the affective elements involved. At least six well-differentiated types of theory can be distinguished within the intellectual group of these theories. They may be characterized briefly as follows:

1. The *facilitation* theory assumes that recognition is due to the augmented ease with which a recurrent content comes to consciousness. This theory is usually furnished with an alleged physiological basis, the assumption being that, in consequence of practice, cerebral activity becomes more easily excitable and brain paths become more readily conductive.

Two objections may be urged against the facilitation theory. First, it helps us not at all on the side of consciousness. While it seems probable that brain processes are facilitated as they are repeated, we are chiefly interested in determining what is present to consciousness during the various stages of this habituation. Second, the most facile processes,—the processes which accompany those daily, habitual experiences to which our reactions are all but automatized,—do not bring with them the peculiar experience of familiarity.

2. The *association* theory makes recognition depend upon the arousal of images which on a former occasion have been associated with the

object recognized.<sup>26</sup> There are two well-marked types of association theory: *a.* the *comparison* theory, which supposes that when a stimulus is recognized, a comparison, conscious or unconscious, has taken place between the perception and revived imagery of former experience; *b.* the *fusion* theory, which bases recognition on an amalgamation of a present percept with a simultaneously aroused image of like nature. The *fusion* theory finds a variant in Semon's assumption (38) that the perception of any known object arouses a memorial trace of its former impression, the perception meanwhile having its own intensity peculiarly augmented thereby.

In criticism of the association theory, one may appeal to our experimental results, which furnish hundreds of instances of recognitions in which no associated imagery was aroused. The 'fusion' variant of this theory is more difficult to ignore, since it does not require that the aroused imagery shall become conscious. We should prefer to regard an unconscious process as a physiological one; nor can we see how such a concept of 'fusion' aids in the description of the *consciousness* of familiarity. The fusion theory, moreover, seems inadequate to account for experiences of false recognition, in which case there is no residuum of an identical past experience to be incited to arousal.

3. The *evocation* theory rests on the assumption of separate brain centers for receiving and for storing peripherally aroused impressions, and on the further assumption that recognition depends upon excitations which are centrally, rather than peripherally aroused. Certain advocates<sup>27</sup> of this theory refer psychic blindness to the absence of such centrifugal excitations.

The evocation theory, like that of facilitation, is of no aid in envisaging the conscious components of recognition. Even if one should accept the theory of separate centers as an explanation of the brain processes involved, one may urge against it the two-fold objection, that central excitations of associated centers must be a constant phenomenon of waking life, yet we are not continuously in the flush of familiarity; and that such excitations should be most numerous and intensive (through facilitation) in the case of objects which we hourly encounter. Yet it is not with these objects that we experience intensively the unique consciousness which is commonly called the feeling of familiarity.

4. The *adjustment* theory explains recognition as an awareness of a bodily attitude which is appropriate to an organized motor response. This theory is more nearly in accord with our findings than any one of those previously discussed; but it is not, we believe, entirely adequate. As Katzaroff has pointed out, motor responses become well organized only by habit; and it is just in cases of habitual stimuli that the experience of familiarity is lacking (55, p. 18). Motor adjustment is rather the climax of the experience of familiarity,—an essential component of recognizing; it is not the complete sum-total of the familiarity consciousness.

5. The *relation* theory attributes the 'feeling of known' to an awareness of relation. This awareness of relation is conceived to be a unique mental datum,—a datum which is a product of past experience, and which is dominantly of an intellectual sort. Calkins (46,

<sup>26</sup> This theory is more fully discussed in connection with the Lehmann-Höfding controversy (pp. 314f.).

<sup>27</sup> Cf. Munk (39) and Wilbrand (41).

pp. 130, 131) believes that it consists in an awareness of relation between the object of recognition and the consciousness of 'self' as persistent. Neither our experimental findings nor the writer's own introspections support the view of an 'awareness of self as persistent,' nor was such an awareness an essential component of the recognitions described by our observers. If we understand Ach (45, p. 263) his experience of 'relation' is an intellectual datum, not further reducible, containing neither sensory nor affective elements. Our recognitions sometimes contained affective components and they always included sensory elements.

6. *Mental Attitudes*. This type of theory attributes the process of recognizing to the peculiar mental adjustment which accompanies the perception of a known stimulus. Betz' (47) phrase, the 'adjustment (*Einstellung*) which goes best' seems, however, rather to be an attempt to describe the mental experience at the moment when a sense of familiarity has culminated in an identification. Clarke (48) believes that recognition is a conscious attitude, but maintains, on the basis of experimental evidence, that it can be resolved into various elements, sensory and affective.

The terms attitude and *Einstellung* seem to refer to a given moment of consciousness,—a relatively static experience. We are convinced that recognition,—and its unique and essential component, the awareness of familiarity,—is an experience whose essence consists in the fact that it is in motion, that it is extended in time, and that it consists in habitual sequences of attention.

Two groups of theories may be distinguished among those which we have called the affective theories of the 'derived' group. One of these emphasizes the consciousness of self as a basic component of the 'feeling of familiarity,' while the other regards the act of recognition as a unique affective quality which has come to be attached to the re-experiencing of any mental process. If our data have yielded conclusions which are applicable to all recognitions, neither of these theories has any factual basis.

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## CERTAIN SOCIAL ASPECTS OF INVENTION

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In studying the conditions under which inventions appear, perhaps three things most impress one: (1) the lack of any regular relation between the value of the invention to society and the degree of ability involved in making it; (2) the similarities in the motives of the inventors; (3) the wide differences in their training and their methods of attack and of work.

1. The social value of an invention shows little with regard to the genius required for it. There can be little question, for instance, that Watt's work with the complicated details of the steam engine involved not only greater technical knowledge of the laws of steam, iron, steel, etc., but also more complicated inductive and imaginative processes than such an invention as Gutenberg's of movable types. Movable types were only the last stage of a social process that had been going on for 250 years. As early as 1190 a German mill had been making paper out of linen rags, thus providing a much cheaper material than parchment for writing and engraving. This naturally gave a great stimulus to the reproduction of manuscripts, and in many instances plates were engraved of the entire page of a manuscript and as many copies made as desired. What Gutenberg did, therefore, in 1438, was to form the idea of engraving the single letters instead of the entire page, and of holding them together in some kind of frame while the print was being made. It seems a small thing to bring about the marvelous changes which followed, and we do not know whether Gutenberg himself foresaw its wider effects on civilization, though probably he did realize its immediate value in simplifying the labor involved in printing. It takes far less skill to engrave a single letter than to engrave a page of words, nor would it take long to learn the twenty-six letters of the alphabet, whether for the purpose of engraving them or of setting up copy. In spite of the relatively small amount of originality involved here, no one would question that the printing press has done at least as much for civilization as the stationary engine.

In this connection we think naturally also of the many instances of men who have made inventions and discoveries already made by others. There is no doubt that the spinner Hargreaves invented a spinning jenny in 1764 which spun twenty threads at once. He had as much inventive genius as Arkwright, if indeed he had not more, but he lacked the social outlook. The same holds of the sewing machine. The Frenchman Thimonnier not only invented but patented a sewing machine in France in 1830, before Howe had even begun to think of it, but the mob prejudice which threatened his life on account of it caused it to drop out of use and almost out of memory. In our own country Walter Hunt invented another sewing machine at least six years before Howe did, and sold his right in it to a Mr. Arrowsmith, but neither of them saw the social possibilities, a patent was not even applied for, and the model was relegated to the garret until Howe's success recalled this earlier invention to mind. We know that Bell has become rich and famous for inventing the telephone because his application for patent rights reached the patent office in Washington some twenty-four hours sooner than Gray's did.

Of such and many similar cases it is as trite to say that the unsuccessful man had as much ability as the successful, as it is superficial to depreciate the genius of the successful man because others have done the same thing. The interesting and important problem both for the psychologist and the sociologist is to ascertain if possible what other factors, whether individual or social, are involved which lead to the final success or failure, and how the waste of duplicating work can be avoided in the future. A knowledge of the lives of inventors and the conditions under which they lived should be at least suggestive, although it must be admittedly more or less defective in the details.

One group of inventions which shows the constant interaction between the individual inventors and social conditions is that concerned with making clothes, including the spinning jenny, cotton gin, power loom and sewing machine. If there were time and space doubtless we could carry these back to the dawn of civilization, but we will begin rather arbitrarily with Kay's invention of the flying shuttle in 1733. Before this a sort of equilibrium had been reached between the weaving and spinning processes. One spinner could hardly supply enough thread to keep one weaver busy, but this made the price of thread high enough to encourage spinners and discourage weavers, so that spinners increased enough in num-

bers to supply at least a fair amount to the weavers. Then came Kay and upset all this by his shuttle, which would send the thread through a warp of any width and do it twice as rapidly as before. The inevitable result followed. The weavers could do twice as much work, and wanted twice as much thread, but the spinners could not supply the thread for two reasons. In the first place they were working as fast as they could before, and in the second place, not many additional spinners could go to work because it was difficult to get more raw cotton. As a result the price of thread increased greatly, and the weavers were obliged to be idle one-half or one-third of their time. It is estimated that in 1760 in Lancashire, England, there were some 50,000 spinners and as many weavers as could find thread for their looms, but the looms stood idle much of the time, the price of thread remained high, and the need of relief was great.

Arkwright saw the need and, barber though he was, set about satisfying it in order thereby to make his own fortune. He was not the first to try to spin by machine, nor was he himself a machinist. Indeed we can not even affirm positively that he was ignorant of Wyatt's invention of 1733 or of Hargreaves' jenny of 1764. We are not even sure how much is owing to the machinist Kay whom he engaged to make his models for him. The most characteristic thing in his life is his clear realization of the need of more cotton thread and the gain to the man who could supply that need. In the invention itself, the most difficult part was to draw the cotton out to a thread by machine. Arkwright tells us that he finally got his idea from seeing red hot iron drawn out between two pairs of rollers, of which one pair revolved more rapidly than the other. So if cotton is put between rollers, the second pair revolving more rapidly than the first pulls it out into numerous threads. This was the central feature of the invention, though many details were added, to avoid breaking the threads, to twist them, to wind them onto spools, and so on. All this took up six years, from 1763 to 1769, in which Arkwright had spent all his money, had nearly starved himself and his wife (who finally left him), and had incurred the popular suspicion of being in league with the devil. In 1769 he took out his first patent and in 1775 a second on an improved model, but the jenny did not come into general use at once because he incurred the hostility both of the spinners and weavers. The spinners burned his mills when they could, because they believed that his invention would deprive them of work, and the cloth-makers organ-

ized against him and agreed not to buy his thread, because they thought he was making exorbitant profits. The long years of suit for infringements of his patent rights ended in 1784 by making the invention free, and the importation of raw cotton into England took a tremendous leap, rising between 1785 and 1790 from about 5,000,000 lbs. per annum to 25,000,000.

It is most interesting to notice however, that practically none of this came from our own southern states. In 1793, our total exports were only 187,000 lbs., and there was no prospect of an increase because we could for the most part only raise the short stapled cotton, which was very difficult to separate from its seeds. A smart negro woman could pick out one pound of cotton a day from the seeds, and would then have six pounds of seeds to the pound of fibre. Arkwright's spinning jenny therefore received checks in two directions and put the pressure on strongly in two places. It was limited by the amount of raw cotton which could be procured, and also by the rapidity with which the thread could be woven into cloth.

Let us shift the scene accordingly to Mrs. Nathanael Greene's Georgia plantation in the years just after the Revolution. At the end of the war our government was so poor that it paid the soldiers for the most part in grants of the new land west of the Appalachians and thus started the stream of migration which drained the eastern coast for years. Georgia, in common with the other southern states was losing its men, and it could offer no counter attractions strong enough to keep them at home. At that time the chief southern products were tobacco, rice, and indigo, and there were vast tracts which were not adapted to any of these, though they were suitable for the short stapled cotton. By 1790 the greatly increased English demand for raw cotton was widely known, but to southern patriots there seemed no prospect of our country benefiting by it because the short stapled cotton was so entangled with its seeds. This aggravating condition was the subject of much thought. It was under discussion at Mrs. Greene's one day by a party of gentlemen, who brought up the question of whether a machine could not be made that would separate the seeds from the cotton. Mrs. Greene exclaimed that if such a thing was possible she knew the man to do it, and she introduced Eli Whitney, a young Yale graduate who was tutoring her children to support himself while he studied law. Her enthusiasm was due to the fact that Eli was constantly mending and making things about the

plantation, some of them pieces of work requiring fine manipulation. He had grown up in a machine shop and was noted wherever he went as very skillful with his hands. Whitney had a long talk with these gentlemen about the profit to the South from such an invention, as well as to the inventor, and was so impressed by it that he got some unpicked cotton and began to experiment with it. He had to make various tools for himself, but in only a few months he had the central idea of the cotton gin in rough working order. The cotton was laid on an iron grating, something like a window screen but much coarser. Below this grating was a cylinder or roller set with projecting teeth in rows, like a coarse saw. When the roller was turned the teeth came through the holes in the grating, seized the cotton and tore it off from the seeds and down through the holes, the holes of course being smaller than the seeds. Whitney tried wooden points first, but found that they broke, and then the saw-like arrangement, but this was also defective and for some time he was puzzled, till one of the children brought him some stiff wire one day to make a bird cage, and he thought it was stiff enough to make teeth for the gin. When he first used this it worked so well that he called Mrs. Greene and others to see the triumph, but after a few revolutions the cylinder became so clogged with cotton that no more was torn off from above. Then, the story goes, Mrs. Greene seized a broom lying near and held it firmly against the roller as it turned, so that the cotton was brushed off, thus completing the invention. Whitney made this a permanent part by putting in a second roller covered with brushes, which revolved in front of the first and cleaned it as it filled with the cotton. This was the first cotton gin. It ran by hand, but it cleaned fifty pounds of cotton a day instead of one. Within a short time, however, Whitney enlarged it to be run by two horses, and to clean 500 lbs. a day, and later on it was run by water or steam power on a still larger scale.

The news of the wonderful invention spread far and wide. It went into use so rapidly that while in 1793 only 187,000 lbs. of cotton were exported, 1,600,000 were exported in 1794, a year later, 6,276,000 and in 1803, only ten years later, 40,000,000. Here, too, the inventor at once became involved in disputes over his patent. Many men succeeded in stealing the plans of the gin and making it for their own use. Whitney's factory was burned, and his patent right was defectively worded so that it was easy to evade. He could not even get witnesses to testify that his gin was in use in



Georgia, though in one case three were in operation so near the courthouse that the noise could be heard there distinctly. He brought suit after suit, nevertheless, spending there what money he received from other states, and finally after thirteen years of litigation he obtained a favorable decision, but was not able to get an extension of his patent right for a second term. Whitney himself therefore never made much money from his invention, and yet the judge who rendered the final decision said that the cotton gin had paid the debts of the South and trebled the value of her land. Surely this was a remarkable effect to come from four or five months of work.

Let us turn back now to the other check upon Arkwright's spinning jenny, that is, the inability of the weavers to use all the thread that could be spun. When the final decision was reached against Arkwright's patent letters, so that anyone could use the spinning jenny without paying for it, it became a burning question among business men whether to put up thread factories or not. A party who were dining together were discussing this one day in 1784, and reached the conclusion that thread factories would be risky investments, because weavers could use only a small part of the thread that could now be made. The clergyman of the town, Dr. Cartwright, who happened to be with the party, remarked that the only thing that would be satisfactory to every one concerned would be for Arkwright to invent a weaving machine that could go fast enough to use up all his yarn. This was very amusing to the manufacturers, who demonstrated to their own satisfaction how impossible it would be for a machine to make the movements necessary, in answer to which Dr. Cartwright could say nothing but that if a machine could play chess, like the one then on exhibition in London, another machine could certainly weave, which was a much simpler operation.

This conversation left such an impression on Dr. Cartwright's mind that he determined to try his hand at making such a machine, though it would have been hard to find anyone less fitted than he for such work. In the first place he came from a gentleman's family, in which work with the hands was considered degrading, and in boyhood he had never shown any liking for tools. In the second place, he had already acquired considerable reputation as a poet and writer, and had clearly before him affluence and reputation in those lines.

So ignorant was he of weaving that he had never seen a

loom in operation, nor did he desire to see one, but set to work *de novo*. He became more interested as he went on, engaged a mechanic to work out his ideas, and in the course of a year had a loom which he thought wonderful. He proceeded to take out a patent on it, and found to his intense mortification that his loom was not nearly so good as those already in use. Then he condescended to study what others had done, and set to work again. This time he worked for two years, and in 1787 he patented a loom in which the picking, shedding and beating up, formerly needing the hand, were done by machinery. This went considerably faster than other looms, though it needed one weaver to each loom, but its great defect was that if a thread broke the loom could not be stopped quickly, and so the cloth was likely to have holes and bunches. Cartwright worked in vain to remedy this, and not until 1841 was a device made by another man that was successful. Up to that time the hand loom competed with the power loom with considerable success, because its cloth was so much better in quality. About the time that Cartwright took out his second patent, other men took out a patent on a method of sizing, or stiffening the warp before it was put onto the loom. This made Cartwright's invention far more practical, because the warp was easier to handle and less likely to break. With this addition a child of fourteen could run two looms and weave three and a half times as fast as the hand weaver.

Cartwright met the same opposition as Arkwright. His mills too were burned by mobs, and his patents were infringed. He spent all his money in the invention, and finally went into bankruptcy and had to start life anew in his old age. In his case however, the Lancashire cloth manufacturers petitioned the Lords of the Treasury to reward him for his public services, and they gave him £10,000, which freed him from financial worries. His mind seems to have literally rioted in inventions in his later years, many of them useful ones, and it is interesting to know that he was one of the men who gave the most encouragement to Robert Fulton.

These three inventions therefore made cotton cloth cheaper than it had ever been in the history of the world, but the social pressure was only shifted to another place, viz., to the making of it into clothing. The abundance and cheapness of cloth naturally made everyone desirous of more clothing, but the amount of clothing was strictly limited by the rapidity with which sewing could be done by hand. The pressure was

felt in every kind of clothing but perhaps especially in the difficulty governments had in equipping their armies properly, both with clothing and with tents. In the seventy-five years preceding Howe's sewing machine patent of 1846, various patents had been taken out in France, England and our own country, for embroidering, quilting and knitting by machine, and we have already referred to Thimonnier's patent of 1830.

There is no evidence that Howe knew of Thimonnier's invention. He seems to have got his inspiration from a conversation which he overheard between his employer, a mechanic, and some men who had come to get his help in contriving a knitting machine. Mr. Davis examined their model but asked them why they bothered with a knitting machine when a sewing machine would be so much more profitable. They said that it would not be possible to make a sewing machine; to which he retorted that he could do it himself without half trying. The men told him he would become both rich and famous if he did, but apparently he did not care to, nor did Howe for several years, though he did think about the possibility of it. He was then a boy of 18 or 19, poor and sickly. When 21 he married, and both his sickliness and poverty increased steadily, so that he was incompetent in his work and wretched. In very desperation to relieve his poverty he began in 1843 to experiment seriously on a model for a sewing machine. At first he assumed that the needle in the machine must make much the same movements as when held in the hand, and he seems to have spent something like a year in fruitless attempts to imitate the hand movement, although he did alter the needle, experimenting with a needle that had two points and the eye in the middle, and trying to invent a device that would push the needle back and forth through the cloth. At length he asked himself why he should try to get a stitch like the hand stitch? The necessary thing was only to hold the two pieces of cloth together firmly. After he had thus freed himself from too close imitation he progressed rapidly, so that in a few months he had the idea of the lock-stitch worked out on his rough model. In the lock stitch the eye of the needle is as close as possible to the point, so that when the point is pushed through the cloth the thread is carried through and makes a loop on the lower side. A shuttle carrying thread is then thrown through this loop, the loop is drawn tight and the first stitch is made complete.

This first wood and wire machine however, was far from being good enough to convince others of its commercial value,

but Howe was so sure of ultimate success that he gave up his position and took his family to live first with his father and then with Mr. George Fisher, who believed in the machine enough to become his partner, and who was willing therefore to support Howe's family and advance him money. About six months later his model was good enough so that he sewed his first seam on the machine, and a few weeks later, in May of 1845, he sewed two suits of clothes on it, one for his partner and the other for himself. This first model was a very little thing that would fit into a box with a cubic capacity of only one and one-half feet. It was turned by hand, but it sewed 250 stitches a minute, about seven times as fast as the most rapid seamstress, and we all know how much firmer the machine stitch is than the hand stitch.

Howe tried in vain to get a tailor to exhibit the machine in Boston but finally did it himself, sewing anything brought to him and racing with any tailors or seamstresses who were willing to do so. Still no orders for the machine came in. One reason for this was that it cost \$300, a price so high that it could only be used in large tailoring establishments, but other reasons were the belief that it could only sew straight seams, and that it would deprive tailors and seamstresses of work. In September, 1846, he took out the patent on his machine, and for another year struggled on against indifference and ever increasing poverty. Then as a last desperate venture his father bought a steerage ticket to London for Elias's brother Amasa, in order that he might try to interest some wealthy man there. Amasa succeeded in rousing the interest, but unfortunately the corset-maker in question was willing to take advantage of his dire poverty and his inexperience, and for £250 he bought the right to take out the English patent, to own the model Amasa had brought with him, and to make and sell as many as he pleased. He agreed to pay Elias £3 on every machine sold in England but never did so, though he made for himself a fortune of over \$1,000,000. Then he persuaded Elias to come over in order to adapt the machine to corset-making, but after he, with his wife and three children was settled in London, there was constant fault-finding and finally a break. Elias was discharged and left practically penniless in a strange land. The family nearly starved to death literally, and at length Howe borrowed money to send them home in the steerage, while he stayed on to finish his third model. His own situation became more desperate all the time, however, and at length only by pawning his patent letters was he able to get money

to buy himself another steerage ticket to New York. He landed in this country with twenty cents, and his biographers tell us, as weak from lack of food as if he had had a severe illness. Within a week he received news that his wife was dying in Cambridge, but he was unable to go until his father sent him enough money to pay for his ticket, and in order to attend her funeral he had to borrow a suit of clothes.

Now, as with other inventors, he found that the sewing machine was coming into favor in this country. Other men had also made machines, and though there is little evidence that they copied from Howe, they did in most cases infringe his patent. He tried at first to negotiate with them privately, after recovering his patent letters from the pawnshop by means of borrowed money, but under the leadership of Isaac Singer they refused to treat with him, and he either had to bring suit or lose all benefit from the invention. His father again came to his aid, mortgaging his farm this time, for \$2,000. In the course of three years the matter was decisively settled in Howe's favor, and when his first patent expired he was able to renew it for an additional seven years, so that at its expiration the machine had earned for him about \$1,700,000.

The modifications of the machine are almost infinite. Between 1842 and 1895, patents to the number of 7,430 were taken out on various accessories. It is used not only for sewing cloth, but also shoes, harness, corsets, rubber hose, belting, etc. It is now run usually by steam, and instead of 250 stitches per minutes can sew 900. In some instances a considerable number of needles (two to twelve) is fastened to one machine so that several seams can be sewed at once.

These four inventors show in a very interesting fashion the possible individual variations in invention, together with the interdependence of inventions and general social conditions, and the combination of motives which influence each individual. All four had a strong conviction of the social value of their inventions but the other personal factors were very different. Arkwright saw the future of the spinning jenny, and the wealth to be realized from it, but the extent of his mechanical genius is a question. He had instead the power of choosing and controlling men, which not only procured for him the first use of the invention but later on enabled him to organize his factories so well as to become wealthy in spite of the loss of his lawsuits. From the available data one is inclined to think that he was far more the able administrator than the inventive genius. The spirit of

two men could hardly vary more than did his and Cartwright's. The latter's chief, if not sole motive, was to demonstrate the possibilities in a machine, although he realized also what its value would be to society. He had, however, no such personal ambition as Arkwright, and was more likely to lose in social position as well as wealth than to gain by going into such mechanical work. It is difficult to understand just why he should have taken up the task of making a loom with such ardor, when he had never before shown any aptitude for tools, but the outcome seems to show beyond doubt that he had very unusual inventive genius. His first crude loom is itself a remarkable achievement for a man who had never seen one, and the variety of his other inventions, many of which he gave freely to other men instead of patenting them himself, shows his breadth of imagination. In Whitney and Howe, on the other hand, especially Whitney, we get men who grew up in the machine shop, but while Whitney had a great deal of administrative ability, Howe was so hampered by his feeble health with its consequences of inefficiency and self distrust that it was difficult for him to convince others of the value of his invention. It would be very interesting to see how far these men conform to Adler's theory of organic *Minderwertigkeit*, but it can not be attempted here. All save Cartwright assuredly had the sense of poverty and social inferiority, while Howe had in addition a very weakly constitution.

Another chapter that would be well worth while psychologically would be a study of the motives that led manufacturers to the attempts to infringe the patents on the one side, and the obstinacy of all four of these inventors in fighting such infringements on the other side. This however might well be extended to all inventions. So notorious is such infringement that we are told (in a recent number of "Science") that Edward Weston protected himself by not taking out a patent on a given device until he had an improvement on it so far along that he could take out a patent on that also within a short time, and thus balk the numerous manufacturers who always take advantage of the description required in the application for a patent. Manufacturers on the other hand, are so well aware of the possibilities of stealing inventions, that if one of their men invents a device they often prefer not to take out a patent but to have the machines made secretly in their own factory and keep them under lock and key, paying the men who use them liberally enough to secure their loyalty. Nor do they scruple to send their own employes

into the factories of others to pick up what they can in the way of new devices. It is not uncommon for them to make a contract which gives them the right to all of a man's inventions for a term of years.

It is not difficult to understand the feelings on both sides in the matter. The inventor naturally enough feels that he himself has created the machine, and has the first right to whatever it can earn. In some cases, as with Howe and Morse, who underwent great privations in the course of their work, most of us must feel that no amount of money can be an adequate compensation. The inventor who has had to endure for years the distrust and criticism of friends and perhaps of his own family, to say nothing of poverty, naturally feels that he has a right later on to any kind of compensation he can get, although nothing can really heal the wounds made by the ridicule and scorn of his social world.

On the other hand the manufacturers have considerable reason for feeling that it is more or less accidental that this or that individual was the first to make or at least the first to *patent* a given machine or device, and that the rewards for the invention are often all out of proportion to the work and talent involved. Whitney, e. g., worked only a few months on the gin and spent very little money, but in introducing the gin through the South he took one pound out of every three cleaned by way of payment. In the contract made with the state of North Carolina it agreed to pay him two shillings and six pence a year for every saw used in the state. That is, Whitney would not sell a single machine, but would only allow the use of them under strict conditions of supervision, which irritated the planters and inclined them to take any possible advantage of him. Not only that, but the planters were poor men as a rule, and probably felt that their need was made the occasion for extortion.

Again, in Whitney's case no one could question that he had made the invention, but in Arkwright's the fact that he was unable to make his own models almost inevitably throws doubt on his inventive ability. In the actual process of invention the relation between manipulation and originality is so close, the chance of an accidental, unforeseen success or failure in combining the various parts of the machine or the various materials used is so great, that it is small wonder Arkwright's competitors questioned whether he or his machinist or Hargreaves should have the credit. Again, we have shown repeatedly that various men have made the same invention, or that the final step to be taken was a very small one.

Howe simply happened to be fortunate in taking out the first American patent, as did Bell. Everyone knows that Wallace formulated the same theory as to the origin of species as did Darwin, and that both got the germ of their idea from Malthus. The one factor in each case which gives one man sufficient advantage over others so that he gets most of the fame and the money for the invention seems more often than not to be entirely unrelated to the genius or talent necessary. In Arkwright's case it was his rather bullying disposition and his administrative ability, though his patent letters were so defective that he finally lost his lawsuits. In Howe's, on the other hand, it was solely due to the fact that he took out his patent as soon as possible, while he seems to have been totally lacking in the ability to impress and manage men so conspicuous in his most prominent opponent, Isaac Singer. Whitney finally made his fortune in the totally different line of gun-making, after spending most of the money that came in from the gin in lawsuits; while Cartwright again, after similarly spending his fortune on lawsuits lived on a government pension and gave away many of the later devices which might have made him wealthy. These men seem to have secured recognition and to have been motivated by their sense of the social value of the invention, though this was not sufficient to secure wealth to all of them.

But again, if we had access to the inner history of factories, there is no doubt that we should find that many workmen make devices or inventions which have far-reaching effects on the industry without themselves foreseeing the effects. The short cut appears in the course of working with the machine, and the workman may use it himself for weeks or months before the foreman or manufacturer notices it. Then only too frequently the manufacturer adopts the device either without giving the workman any credit or money payment, or giving him a sum very small in proportion to the saving of money or labor made by the device. So we have on the one side factories offering rewards of small sums for inventions, and on the other side workmen refusing to think or plan, or concealing their improved methods, or when they are shrewd enough carrying them to a competing firm to sell.

It would appear therefore, that while the actual attainment of fame and fortune depends upon the social insight of the inventor impelling him to take out patents, and while in many instances the value to society and the desire to improve his own position and fortune are the motivating factors, the mechanical ability or inventive process itself goes on in many



other cases without any apparent relation to society, but more as a by-product of the interaction between the workman and his machine.

Rignano in his lucid series of articles has restated the point made by several others that scientific generalization rests in the end upon the possibility of repeating the experiments on which the theory rests, that is, in repeating the sensations and perceptions, so that all normal persons may get the same experience. In proportion as we depart from experiment and proceed to inference and induction we enter debatable ground, for it is increasingly difficult to keep clearly in mind all the data and particulars involved. Nevertheless, the history of science, and especially of mathematical science, which Rignano discusses in his latest article, shows that even the most abstract ideas, imaginary numbers, and the like, though they may be symbols of symbols, run back to a physical reality, to a perceptual experience, including in this the kinesthetic factors and particularly those involved in manipulation.

Granted such a basis of sensation and perception, the part which they play in any creative thought process becomes a highly important problem. To what degree can a man carry on synthetic thought without recourse to perception? What part is played by the process of writing or speaking as contributing perceptual factors? Is the new combination more likely to be attained first in thought or in a seemingly accidental combination of perceptions? In invention we see as clearly as anywhere else, perhaps, the close relation between the perceptual experience and the concept or theory or abstract truth, but also the wide variations possible in the proportions of each. In his *Psychology of Efficiency* Rutgers has analyzed human methods of meeting new situations which involve motor processes. His material consisted of mechanical puzzles, usually of wire, in three dimensions. In order to solve the puzzle some part had to be removed thus necessitating manipulation as well as thought. In his twenty-seven subjects he found that the methods of solution ranged from the animal hit-and-miss method to the forming of a working hypothesis before the puzzle was touched, but in general in proportion as the problem was new the hit-and-miss method was approximated. These extremes remained in some cases even when the puzzle was learned. That is, some subjects, even when they could do it perfectly seemed unable to follow a plan, but did it as it were automatically, while others could describe each step in detail before doing it. When the subject took the puzzle and began to handle it, his movements were

to large degree random, and he might chance upon the one which solved the puzzle. His future success then depended upon the degree to which he could recall and attend to the movements made, and see their relation to the puzzle. Not infrequently memory was illusory, or a false hypothesis had been formed which prevented the subject from experimenting freely and hindered the solution. In the actual solving, there was a complicated interaction between random movements and attention to them. Sometimes the movement was accidental but the subject saw just as he made the movement that it would be successful; in other cases he could remember it well enough to succeed at once the next time. As a rule however the first accidental success served only to localize the point of attack, and further experimenting about this focus was necessary. Rutgers, like Book, emphasizes certain points in the learning. The first success with the puzzles, like the first short cut in typewriting, is likely to be accidental, but it is also likely to come only when the subject is in the best physical condition and is attending most closely. Then it is likely to be done more or less unconsciously for a time, but gradually to be perceived and be brought under volitional control. Book then goes into further detail in the study of acquiring skill in typewriting. After the first rapid stage of learning, the subject reaches the point of average attainment when it is very much harder to acquire a greater degree of rapidity or correctness, partly because the material has become familiar and so less interesting, and partly because few persons have had any experience in making the higher groupings. The learner tends to become lazy and remain at the same level, or on the other hand he tries to proceed too rapidly, and so fails in correctness as he progresses. To work at the optimum but not to go beyond it or fall below it is the important problem for each individual, and this again is a matter of wise control of attention. Nevertheless, Book emphasizes again and again, that the most effective control of attention is not obtained by direct means, but only by keeping in the best physical condition and by having the keenest interest in the work and the highest incentive to keep at it. He who says, "Go to, to-day I will acquire this or that definite degree of skill," is almost sure to require either too much or too little of himself. The psycho-physical organism is too complex for us to forecast its possibilities at a given time as yet.

This holds to an even greater degree of inventions. While it is generally true that the workman in a given trade is more likely to invent than one who knows nothing of the industry

in question, it is not essential. Even in a trade so technical as engineering Kent reports that of the 72 talented American engineers whom he studied 16% had not come into contact with machinery in boyhood and 17% reported that they had no especial mechanical interests in boyhood. On the other hand Kent says that in all the cases of which he knows, a boy who has made an engine before he is seventeen years old has later attained eminence. That is, the early presence of mechanical interest and skill enables us to prophecy positively, but its absence does not make it safe to prophecy negatively, for it may be compensated later by unusual motivations and application.

Let us take up in more detail a specific invention, that of Watt, to see if possible how accidental manipulations or chance associations interact with the end in view and the working hypothesis. Watt is one of the engineers who showed his taste from boyhood. Adler would probably consider him an excellent case of compensation for organic inferiority. He was a sickly boy, unable to share the active sports of other boys, so that he was forced to amuse himself to large degree. He naturally enough imitated his father's work in his shop, and when fourteen years old he set up a forge for himself. His father sold instruments and also had a carpenter shop and the boy learned to use the various nautical and other instruments on sale or left for repairs. When eighteen years old he determined to be a maker of scientific instruments, and went to London and later to Glasgow to learn the trade. After various difficulties with the guild, because he did not wish to spend the required number of years as an apprentice, the University of Glasgow in 1757 gave him the use of a room and a half official connection with it. Here he made all sorts of mathematical and musical instruments, taking a partner later, and building a flourishing business.

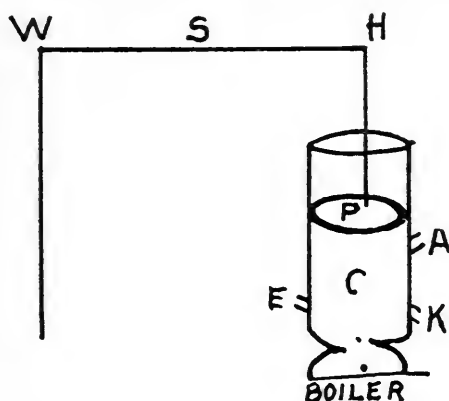
To understand the conditions under which Watt worked something must be said of certain other discoveries upon which the invention of a steam engine depended. Even the Greeks and Romans knew something of the power of steam, and later it was used to a small extent for melting ores, and for turning the spit. In 1663 the Marquis of Worcester described a pump to be run by steam to pump the water out of mines, but could not make his idea practical. Only thirty years later however, the Frenchman Papin discovered that if water is boiled in an air-tight vessel until it all turns to steam, and if the steam is then condensed by sudden cooling, a vacuum will be left in the vessel. Of course no vacuum

is perfect, but in Papin's the difference between the atmospheric pressure and that of the so-called vacuum was great enough to make a crude steam pump practicable, and Savery made this use of the vacuum eight years after Papin's discovery. The principle involved is very simple. There is a large steam vessel, the precursor of the engine cylinder, set over the boiler, with an opening between the two so that the cylinder fills with steam as the water in the boiler boils. The cylinder has three other openings: (1) There is a pipe leading down to the water in the mine, which is to be pumped out; (2) Another pipe leading to discharge the water after it has been drawn up, and (3) a third pipe by which the air is forced out of the cylinder. At first the water and discharge pipes are closed and the cylinder is filled with steam which forces out all the air. The steam is then condensed, and the water pipe opened. Since the pressure within the cylinder is much less than that outside it, the water is forced up into it till the pressures are equal. The water pipe is then closed, the cylinder again filled with steam and the discharge pipe opened and the mingled water and steam discharged.

This however was greatly improved by the Englishman Newcomen, who had a very pressing problem before him. In England the need of power for pumping the water out of the copper mines had become acute. The mines were extremely valuable, but they had been worked for a long time and had become so deep that the enormous suction pumps run by horses would no longer pump out the water. Many had been abandoned although the copper was by no means exhausted and additional ones were being closed all the time. Newcomen formed the idea of using Savery's pump with improvements. In the first place he wanted to use the pumps already in the mines, but to have a greater power attached where the horses had been before. That is, he wished to make steam work the handle of the pump up and down.

This necessitated a number of changes which can only be indicated here in the rough diagram. From W a chain runs down to the pump in the mine, and HW is a beam hung on a socket at S. From H a rod runs down to the piston P, which is essentially a smooth sliding cover to the cylinder, C. When the steam is forced up into C, the air is driven out through A as in Savery's engine and when the cylinder is full of steam the boiler is shut off from it, and the steam is condensed by a spray of water running in from E, the water being run off from the cylinder through K, leaving the vacuum in C. Then the pressure of the air outside the

piston forces the piston down at once, and with it the beam, which thus begins to work the pump at the other end. When the piston reaches the bottom of the cylinder it is drawn up to the top and the process begins over again. Newcomen's engine was improved by Smeaton, and for nearly a century it was used commonly in mines. But only 10% of the power was available for the pump, the rest being wasted in various ways. The piston made not more than three or four strokes per minute, and moved about fifty feet per minute, and the engine consumed twenty-eight pounds of coal to each horse power produced, though to-day but one and one-third pounds are needed per horse power. Wasteful though the Newcomen



engine was, it had considerable sale among mine owners because it was the only source of sufficient power to remove the water from the mines.

By 1760 however, it was failing to do even this in some instances, and when young Watt discussed steam with his friends we can not doubt that the deficiencies of the Newcomen engine and the practical need of more power were frequently touched upon. In any event, shortly before 1763 Watt learned that the University had a Newcomen model which had been sent to London to be repaired, and he put in a request that he be allowed to get it back and try his hand at it. This was granted and after a long delay the model reached his workshop. We can reconstruct to some small degree at least, the effect it had upon him. He was a maker of mathematical instruments and that means that he had a great love of exactness combined with remarkable deftness. From childhood he had taken his toys to pieces and not only

put them together again but often had combined the parts in new ways. When he got his forge in his fourteenth year, he made miniature cranes, pulleys, pumps, and so on, and out of a silver coin he made a punch ladle, still preserved. He also mended the various nautical instruments brought to his father, such as compasses and quadrants. Later when established in his rooms at the University of Glasgow, while waiting for a better class of work to come, he made musical instruments, and constructed a barrel organ for the first time, while in the second one that he made he added a number of improvements. He said of himself that he was always dissatisfied with others' work as well as with his own. It is easy to see how so wasteful a machine as the Newcomen engine would irritate a mind of this type, and almost inevitably lead him not only to mend the model given him but to experiment with it to prevent so much waste. Further, in 1759, when Watt was only twenty-three years old, his friend Robison, with whom he was continually making experiments and having discussions, suggested to him that steam might be used to drive carriages and Watt began to make a model of a steam engine, but was not very successful and laid it aside when Robison left Glasgow. Discussions with another friend, Dr. Black, however, kept the idea of steam power alive in Watt's mind, so that when he learned of the Newcomen model he made the request already noted. While waiting for it he experimented on a very small scale with the force of steam, using Papin's digester, and for cylinder an ordinary syringe only one-third of an inch in diameter, with a solid piston and stop-cocks put in to admit or shut off the steam. Tiny and crude though this apparatus was, he learned from it the expansive force of the steam upon the piston, and also the greatly increased elasticity and power of steam kept under high pressure. He saw the mechanical possibilities of this at once, but he could not make actual use of them because he could not get workmen skillful enough to make boilers that would be safe. When the Newcomen model finally arrived and was set up, as he supposed in perfect condition, after only a few strokes of the piston it stopped and would not begin again, though the fire was made as hot as possible and as much steam as possible produced. This was a challenge not to be refused. During the next year Watt and Robison tried various devices, in the course of which Watt stumbled upon the fact of latent heat, which his friend Dr. Black had already discovered and explained the theory of to him. Watt found that steam would heat six times its weight of water

from freezing point to boiling point, its own temperature. About the same time that he was making these experiments (his own accounts and Dr. Robison's, written some twenty years afterwards, do not give us the exact order of events, and differ in some instances with each other), he was also experimenting on both the boiler and the cylinder of the engine. The knowledge of the latent heat in steam made it very important not to waste any steam, and also to increase the heating surface of the boiler as much as possible in order to produce the greatest amount of steam from the smallest amount of coal. Accordingly he ran flues through the boiler, or built the boiler about the fire, made it of non-conducting materials, and so on. By degrees however he became convinced that the greatest waste was in the cylinder, and that in his small models the wastes were greater than they should have been according to the text book accounts. Particularly he found that too much steam was condensed in the cylinder, and that when he tried to condense it more rapidly by injecting more water, too much steam proportionately was wasted. After trying this out repeatedly, he found that water in a vacuum boils at lower temperatures than under usual atmospheric pressure and so he got steam and pressures which he was not expecting. In all this he was using a very small scale of models. Sometimes his cylinder would have a diameter of six inches and a stroke of twelve, or sometimes it would be only an apothecary's syringe, and a flask that would hold a pound of water. Again, a tea kettle would serve as his boiler. But by means of such crude and small apparatus, by the fall of 1764 Watt had reached certain conclusions. He was confident that the chief waste in the Newcomen engine came in cooling the cylinder. We have already noted that to get a vacuum in the cylinder, the air is first driven out by steam, and then the steam is suddenly condensed from  $212^{\circ}$  to about  $100^{\circ}$  by the injection of a stream of water. This of course also cools the cylinder, and when the steam is let in next time Watt found that from three-fourths to four-fifths of it condensed on the cold walls of the cylinder before any could be used in work. How was this waste to be prevented? Naturally, if the walls of the cylinder could be kept as hot as the steam, the steam would not be wasted in heating them. But how could the steam be condensed without cooling the cylinder? To cool the steam in the cylinder from  $212^{\circ}$  to  $100^{\circ}$  without cooling the walls of the cylinder at all was therefore the crucial problem in the invention of the modern steam engine.

Several months passed of which we know little. In the summer before Watt had married, and during this fall he was attending diligently to his proper business of instrument making, as well as his experiments with the engine models. In the following spring, probably in April of 1765, he went for a walk one Sunday afternoon on the Glasgow Green, and between the unromantic buildings called the wash-house, where the Glasgow washerwomen did their weekly rubbing, and the herd-house, the great idea flashed into his mind. We know no more of the details than that he had been thinking as usual of his engine. This central idea was that, to use Watt's own words, "as steam was an elastic body it would rush into a vacuum and if a communication were opened between the cylinder and an exhausted vessel, it would rush into it and might there be condensed without cooling the cylinder." It would then be necessary to draw off the condensed steam and the injection water from the condenser, but this could be done by a pump. He concludes, "I had not got farther than the golf-house when the whole thing was arranged in my mind." We might say carelessly that now the invention was made, but Watt still had four years of disappointment ahead of him before he could even take out his first patent, and ten or twelve before he began to get any financial returns. Watt was twenty-nine at this time.

In his own account, written thirty-one years later for his attorneys, he says that if he had been content merely to make the engine on this plan, thus saving the steam and fuel, he might have put it on the market very soon, but he wanted to make the cost less as well, and he very soon had in mind other devices which he believed would improve the engine more than they actually did. He says that his inexperience in "mechanics in the great" led him astray.

The next step was to see whether the idea would actually work. The next day Watt obtained a large brass syringe,  $1\frac{3}{4}$  inches only in diameter and 10 inches long and put a tin top and bottom onto it to make it into his cylinder. The condenser consisted of two tin pipes only about  $1/6$  inches in diameter and 10 inches long, standing upright and joined at the bottom to the pump and all placed in a vessel full of cold water. A pipe, of course, ran from the condenser to the cylinder. In the piston rod he also bored a hole through which the water condensing when the cylinder was first heated could come, and when steam began to come out through it and through the condenser valve it was supposed the cylinder was full of steam. The valves were then shut and the vacuum



was made in the condenser by means of the pump, so that the steam rushed in from the cylinder. The piston of the cylinder at once rose in its turn, raising with it a weight of 18 lbs. attached to its rod. (The diameter of the piston was  $1\frac{3}{4}$  inches.) The amount of steam used and the weights lifted were carefully noted.

At first, after the piston had risen it was allowed to fall by the atmospheric pressure on its top, when the vacuum was made in the cylinder below, but Watt realized very soon that the air going down the sides of the cylinder also cooled it (the old cylinder being open at top and air usually being only  $70^{\circ}$ - $80^{\circ}$ ), and that it would be very desirable to close the cylinder at the top and have two pipes at top and two at bottom, to bring in and carry off the steam above and below the piston, pushing it down as well as driving it up by the steam pressure. The engine thus became a true steam engine as distinct from Newcomen's atmospheric engine and the piston a double-acting one. Thus a further saving of heat was effected. This modification seems to have been made even in the first tiny model, and in the larger one which Watt at once made there was also an outer case to surround the cylinder and be filled with steam, in order to keep its heat still more uniform.

Now Watt entered upon what was perhaps the most trying period in his inventing. He himself, as we have noted, was a maker of scientific instruments, with all the exactness of mind and skill of hand which that necessitates. The success of his engine depended entirely upon the presence of those qualities in the workmen who were to make its various parts, especially the cylinder and piston, but he could find no such workmen in the United Kingdom. The larger model which Watt himself made with the aid of a mechanic was good enough to *promise* great things, but the cylinder was imperfect so that the steam escaped at many places and the piston was far from air tight. The same was true of the still larger one which he built a little later, and also of those tried after he went into partnership with Roebuck. The difficulties with the imperfect shape of the cylinder, it should be said, were never overcome until years later when Mr. Boulton found the iron-master John Wilkinson. As long as the cylinder was imperfect the piston could only be relatively tight in it, though Watt did improve this to a considerable degree by the arrangements of collars which he put around the piston, the use of greases and oils, etc., with all of which he seems to have tried almost everything imaginable. From 1765 till

1770 he could not give as much time to the engine as he wished, but took up surveying as a business, and also made minor inventions along other lines. In 1767 he became acquainted with his future partner, Mr. Boulton. Mr. Boulton was the owner of the most famous metal works in England, and for some years he had been greatly interested in the possibility of using steam power in his factories, since he had not sufficient water power for the growing industry. He and Watt had for three years interchanged letters, and debated the possibility of a partnership, but Watt was already bound to Dr. Roebuck so that Mr. Boulton could not enter on the terms that he wanted. In 1770 Mr. Boulton asked Watt to send plans for an engine, which he would try to make at his works. This was done but the engine was no better than the others, though Boulton had the best mechanics in England working for him. Other attempts about the same time with older models, were equally failures and Watt began to think that his engine would never be a commercial success because he could not get skilled workmen to make it. Four years more passed in earning a living, suffering from ill health, and failing in his attempts to improve the engine, but in May of 1774, an agreement was finally reached with Boulton and Watt betook himself and his models to Birmingham. He had taken out a patent in the spring of 1769, but he had not as yet made a single engine for sale, although he had spent several thousand pounds in experiments besides all his labor.

At Soho Boulton's workmen set up the model over which Watt had worked so long at Roebuck's, and this time it worked better than ever before, though still open to much improvement. Boulton also began to get inquiries as to the nature of the engine, whether it could be used for mines, etc., and other men began to steal the plans of the engine or to invent on their own account. All this showed the demand and the financial gain to be made if the patent rights were protected. Boulton accordingly persuaded Watt of the necessity of getting an extension of time on his patent and after some trouble this was secured in the winter of 1775, for twenty-four years.

We should note at this point the invention of John Wilkerson, the iron founder, who introduced a new boring machine to make the large cylinders necessary for cannon and steam engines more accurate. The usual method at that time was to have the rough shape of the cylinder cast solid, and afterwards to bore the size wanted, but there was nothing to guide the borer, and so the hole was not likely to be perfectly straight. Wilkinson however, steadied the borer by a very

strong iron rod, along which the borer slid, so that whatever the shape given in the casting, the hole bored was perfectly straight and round. Nevertheless, when the first engine was made at Soho, Smeaton, perhaps at that time the most famous engineer in England, did not believe it would ever be practical on account of the difficulty of getting the various parts accurately enough made, although he fully recognized its superiority over other forms if this could be done.

Nevertheless Boulton and Watt began to sell engines, guaranteeing that their engines would do as much work as the ordinary engine, for half the expense in fuel, and in March, 1776, Boulton wrote that if they had a hundred small engines and twenty large ones on hand they could easily sell them all. He hoped to make one engine every two or three weeks, or from twelve to fifteen reciprocating and fifty rotative engines per annum. While Boulton managed the business end, Watt was occupied in making improvements and in setting up those sold. He was constantly trying new materials for collars or lubricants to the cylinder, changing the form of the condenser so that it would cool the steam more rapidly, changing the position of the valves and making them tighter, and so on. In one of the first engines made at Soho we are told that it made 500 strokes with 2 cwt. of coals, while a month later it made 2,000 strokes with 1 cwt.

For some years Watt spent most of his days setting up engines, and from 1780 to 1785 he took out five patents on different inventions: a new method of copying letters, a machine for drying linen and muslin by steam; the very important one on methods of applying the power of steam engines to produce a rotary motion around an axis, which made steam applicable to all mill machinery; an additional patent on the steam engine itself. In this last are specified the important principles of using the expansive power of steam, the double-acting piston, the double engine, a toothed rack and sector to guide the piston movement, and a rotative engine or steam wheel. Both the expansive power and the double-acting piston had been known to Watt from about 1767 but he had not used them on account of the difficulties involved in making them. Finally, Watt patented methods of converting circular or angular motion into perpendicular, along with many minor improvements, and a device by which the steam engine would work a tilt hammer, of which combination he writes with great pride in 1783 that the engine had a cylinder of 42 inches diameter, and a stroke of 6 feet, making as much as 50-60 strokes per minute. It would work a

hammer of  $7\frac{1}{2}$  cwt., raised 2 feet high, which could be made to strike 300 blows per minute.<sup>1</sup>

Boulton and Watt got their profits from their engines in the saving made over the common ones, that is, one-third the saving of fuel to be paid either annually or semi-annually. They found so much tendency among the men to steal the coal and to deceive them, that Watt set to work upon a counter, which should record the number of strokes made by the engine and thus enable them to calculate the amount of steam made and coal burned. Among other improvements were those of the throttle valve, governor, steam gauge, indicator, and smoke consumer, which also burned the fuel more completely.

During these years of invention, as before, Watt suffered a great deal from ill health, severe headaches, and low spirits. He speaks of his health being so bad that he fears he can not hold out much longer, he longs for a rest for his weary bones and laments the loss of the inventing power, although it was really as active as ever.

As to the inventive process itself the reader of his biographies is most impressed by the fact that the actual making of an engine valuable for practical purposes lagged so far behind the successful model. The theory was well worked out and the skill in making small models was acquired by Watt ten years before a large working engine could be turned out. Watt himself never seems to have set about acquiring the skill necessary for casting perfect cylinders, nor to have turned his inventive ability upon that problem. This is rather strange, for as far as we can judge from his letters he realized that the difficulty lay in getting a perfect cylinder. Or perhaps we should say that he realized that the difficulty lay in getting the piston perfectly tight in the cylinder, and thought that he might accomplish this by means of his collars and lubricants. At any rate he seems to have expended his energy in that way and to have used the hit-or-miss method to the extreme there. In trying out different shapes for the condenser and boiler, the part played by theory was more important, for he wished to get as much surface as possible for the water or the fire to play over.

It is an interesting question whether, if Watt had not found Boulton and Wilkinson, who took charge of the busi-

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<sup>1</sup> To-day a piston travels only at moderate speed when going 1,000 feet per minute in a cylinder three feet long, and makes 166 revolutions. It costs about eight cents to get one horse-power for twenty-four hours.

ness management and of the actual making of the large cylinders and engines, he would ever have succeeded in the practical application of the theory; whether his engine would not have remained merely a model, useful in the laboratory to illustrate various physical laws, but of no use to industry. Boulton made over one of the most important branches of the iron trade in order to get a workable engine, and in order to do that he trained his workmen to a degree of skill and accuracy before unknown either in England or on the Continent. On the other hand, it is altogether doubtful if he would have appreciated the fact that the failure of the engine was due to numerous small defects in its parts which allowed leakage and waste, had not Watt been constantly on hand in the foundries to indicate these defects, and had he not been the one who set up the engines sold and stayed by them until they worked according to the contract. That is, Watt showed the points at which the theory failed in practice because the machine was badly made, while Wilkinson and Boulton wrestled with the intractable material, poor tools and unskilled workmen. If Watt had not had a firm conviction acquired from his success with his small models that his theory was correct, and if he had not also had the exactness developed by his work with fine tools on scientific instruments, he would probably have been unable to convince Boulton of the practicability of his engine.

In his biographies we do not get nearly enough account of the complicated interplay between theory and the manipulation of the models or the actual engines, but the fact that for years Watt himself set up the engines sold and stayed by them until they were in good working order, while during these same years he was adding the important conveniences of the throttle valve, counter, indicator and governor, as well as others less well known, shows how close the interaction must have been.

We are thus able from the available data to outline roughly three stages. We do not know whether he had any especial interest in steam before his discussions with Dr. Robison in 1759. From 1759 to 1763 there was a preliminary period of discussion, intermittent experiment, discovery of some of the laws of steam pressure, etc. This was followed by a more intense period of application introduced by the work with the Newcomen model and culminating in the image of the separate condenser, which within a day was tested in the first tiny model. This model in turn was followed almost at once by two or more larger ones, and in the course of

this close interaction between manipulation and thinking the double-acting piston was invented, and many other things were invented and tried out which did not work, such as a wooden case for the cylinder. As far as we can judge, the constructive aspect, the shooting together of relatively unconnected images into the constellation that we call the steam engine, occurred in a period of only about two months, during April and perhaps May of 1765. It had been preceded by the long practise period beginning in 1759, becoming more intense from 1763 on, and finally culminating in the invention.

Another practice period follows from 1765 to 1774, when he began his partnership with Boulton at Soho. During these years there seems to have been little addition to the theory, but all sorts of combinations of perceptions and images tried with a view to stopping the various leakages of steam and heat. These attempts however were not very successful. The larger engines made worked just well enough to keep alive the belief of Watt and Roebuck but not well enough to try to sell them. Watt's energies were much divided. He was obliged to take up surveying in order to support his family and pay the debts incurred in his experiments with the engine, and he made various minor inventions along quite different lines. During these years therefore he did little more than mark time. In other words, he was practicing with steam, becoming habituated to it, but he was not working with the intensity and concentration which Book found essential for the step up to the next level. He was on one of the learning plateaus.

When he went to Soho however, in 1774, Boulton provided the necessary stimuli for progress. In the first place, the partnership contract cleared up the financial tangles and freed Watt's mind from them. In the second place, under Boulton's urging, Watt obtained an extension of his patent rights for twenty-four years, so that he felt that he would get some financial profit from his invention. Finally, within six months Boulton began to take orders for engines and to guarantee that they would do a certain amount of work, so that Watt was under the pressure of making the engine at least as good as the contract provided for. The effect was immediate and great, as we have already seen, in one case the number of strokes being increased fourfold for one-half the previous amount of fuel. Within these years from 1774 to 1785 we get a combination of practice and invention which would be most instructive had we only the details. Watt was setting up the engines sold, instructing the engineers how to

run them, and constantly making the minor improvements already referred to. His fancy was constantly playing over the engine to note the weak places and possible improvements, and the constructive process again culminated in the patents taken out between 1780 and 1785, some of which were nearly as valuable commercially as the original one.

Had we the biographical data, there can be little doubt that we should be able within each of these larger periods to mark off sub-periods of the same kind, and to establish correlations between the condition of his health and his inventive power. Space is lacking here to detail the course of infringement of his patents, or his recognition of the social need and value of the steam engine. Suffice it to say that he had a clear consciousness of the benefit to humanity, but in reading his life one gets even more the impression that he kept at the work primarily because his instinct for handling things and seeing what would happen from all kinds of combinations was so strong. He fussed over all kinds of devices that turned out impractical and over many things unrelated to steam. One might almost say that his hands had a constant itch which nothing but work with physical things could stop and that with so much play with things the law of chance alone would ensure some useful things being discovered or made. On the other hand, the focussing of his interest upon steam seems to shows his sensitiveness of response to his environment and to this great industrial need of his day. Why steam made so strong an appeal to him we can not say. Did it typify the power of spirit over matter, of mental energy over physical inertia? Was his struggle against his own bodily weaknesses transferred to this plane? This is purely speculation, but it is at least interesting to see how the weakly boy harnessed the strongest force of modern civilization, making mere size and muscle of less account than ever before.

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## GEOTROPISM IN ANIMALS<sup>1</sup>

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I. The word "geotropism" was first invented by Frank, a German plant physiologist, in 1868 (19). The word "tropism" with prefix, helio, light, however, was first used by De Candolle, a French plant physiologist, in 1835 (39, p. 154). Tropism, as you all know, is a Greek word meaning "to turn." Geotropism is, therefore, "to turn" toward or away from the center of the earth or the force of gravity. Some writers use "geotaxis" instead of "geotropism." Taxis, a Greek word again, means "to arrange." According to them, "geotropism" is for plants which are not motile, and "geotaxis" for animals which are motile. But I prefer "geotropism" rather than "geotaxis" for the reason that the reactions of plants and animals, motile or not motile, to the force of gravity are fundamentally the same thing (cf. 40).

Many animals react to the force of gravity; most of them turn their anterior ends away from the center of the earth, and a few of them toward it. The former are called negatively, and the latter positively geotropic. The phenomena are negative and positive geotropisms.

Paramecium, for example, which is one of the unicellular organisms, orients itself with its anterior end upward, and swims in that direction; thus against gravity. Among the coelenterates, geotropism is shown by certain hydroids; by *Antennularia antennina*—a plant-like animal—for example, whose "stems" have a tendency to curve upward, and their "roots" a tendency to grow vertically downward, though the geotropic curvature of the "roots" is not so precise as in the "stems." The "roots," in this case, are positively, and the "stems" negatively geotropic. If you cut off a piece of the "stem," and reverse it, that is, place the "stem" end down

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<sup>1</sup> This popular paper was read by the writer at the sixth annual meeting of the Minnesota Psychological Conference, which was held at the University of Minnesota, March 27, 1914. The text has not been changed since except to add foot-notes.

and "root" end up, it will regenerate "roots" at the lower end of the piece, which having been the "stem" end, would normally have grown upward, and will regenerate a "stem" at its upper end (31, pp. 494-495). Now, the question arises: what makes the animal orient itself toward or away from the force of gravity?

II. For students of physiology all animals, human animals inclusive, are "chemical machines" in which extremely complex processes are going on. Naturally, therefore, physiologists have attacked the problem of orientation of animals by gravity from a purely physical and chemical viewpoint. Ever since Schwarz first took up the problem in 1884, in unicellular organisms, bacteria, *Euglena*, *Chlamydomonas*, *Desmid*, *Paramecium*, and *Spirostomum*, have been subjected to quite extensive experiments for this purpose. Unfortunately, however, different investigators have offered varying explanations for the phenomena, though this problem we feel, is nearly settled now.<sup>2</sup> Limited time and space, however, prevent our dwelling on it here. We shall therefore simply refer to the names of various theories, namely, mechanical theory, pressure theory, resistance theory, and statocyst theory.

The first three theories as applied to unicellular organisms, having been experimentally shown to be fallacious, E. P. Lyon, in 1905, suggested the last theory. He applied to the unicellular forms, the ideas previously advanced by Goltz, Mach, Breuer, Bethe, Loeb, Verworn, and many others to explain orientation in the higher forms. Lyon's view was based on some experimental results, tending to show that the paramecium contains protoplasmic materials of different specific gravities.<sup>3</sup> "For internal stimulation," according to him, "the relation of the parts of the cell to each other must be changed in some way by gravity. Stresses or pulls which occur when the organism is in one position with respect to the vertical, must be changed in another position" (33, p. 429).

To illustrate the point, let us take some experimental examples. Eggs of sea-urchin, annelids, frogs, etc., when strongly centrifuged, separate into three layers, i. e., light, middle, and heavy.<sup>4</sup> There is no doubt, therefore, that dif-

<sup>2</sup> Cf. 24 and 34. The writer is also preparing a paper, "Further studies on the geotropism of *Paramecium caudatum* and a direct proof that the animal contains protoplasmic materials of different specific gravity," in which he will attempt to settle this problem.

<sup>3</sup> The writer's recent experiments, not yet published, on *Paramecium caudatum*, furnish direct evidence to prove that it is the case.

<sup>4</sup> E. P. Lyon was first to show these results (34).

ferences in specific gravity exist in the protoplasm of the animal cells. As has already been pointed out, the view of the statocyst theory of geotropism in paramecium accords with that of higher animals. Some crustacea, *Palaemonetes*, for example, have "irregular grains of sand mingled with other fine mineral particles and organic detritus" "in the basal segment of the antennule," which are the statoliths<sup>5</sup> for the organisms. The statoliths in question play an important rôle in the geotropic orientation of the organism. This has been proved by Prentiss' experiments as follows:

The statoliths are removed from the sacs of *Palaemonetes* by lifting the lid which covers the aperture, and forcing a fine jet of water into the cavity. Most of the sand having been thus washed out, the animals are placed in an aquarium upon the floor of which iron filings have been scattered and are allowed to remain until the iron particles have been taken into the sac in place of grains of sand. The shrimps (*Palaemonetes*) are blinded by painting the eyestalks with a mixture of lampblack and shellac. A *strong* electromagnet is then used. The pointed end of the magnet is held about 3 cm. from the statocysts, at one side of and a little ventral to them. Animals with normal statoliths, if blinded, do not respond at all, and are apparently unaffected by the proximity of the magnet; they keep their normal position, dorsal side up, with the sagittal plane of the body coincident with the direction of gravity. If not blinded, they simply move slowly away from the magnet when it approaches too near. When, however, the magnet is brought into close proximity to statocysts containing iron filings, the dorsal side of the animal is turned, not toward the magnet, as might be expected if the changed position were due directly to the action of the magnet on the iron filings, but away from it. If the magnet is changed to a position on the other side of the shrimp, the turning is in the opposite direction, still away from the source of attraction (39, p. 239). "There is only one explanation for this turning of the body away from the attracting force, and that is a very simple one. Under normal conditions the body of the shrimp is oriented with reference to gravity, and its dorso-ventral axis approximately corresponds to the direction of this force. If the shrimp rotates around its chief axis either to right or left, say 90°, the direction of the pull of gravity on the statoliths is at once changed, and through the medium

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<sup>5</sup> Verworn (47) first suggested the use of the word "statolith" instead of the word "otolith," which is generally used. The writer thinks it is desirable to accept Verworn's term.

of the latter other sensory hairs of the sac are stimulated. As a result, the shrimp turns back in a direction opposite to that in which it was rotated, until it is again in a normal relation to the direction of gravity. The employment of the magnet has no other effect than merely to change the direction of the orienting force. This is no longer that of gravity alone, but the *resultant* of the two component forces, gravity and the pull of the magnet. The animal now maintains its swimming position in reference to this new line of attraction, its dorso-ventral axis coincident with that line, and as a result the dorsal side is turned away from the magnet" (39, p. 240). Thus we can see that the pull of gravity on the statoliths in *Palaemonetes* plays an important rôle in the physiological function of equilibrium.

In connection with this, let us cite another interesting and important example from the same work. In lobsters, the larvae after ecdysis have no statoliths, and may be kept without them for one or two days, if they are placed at once after ecdysis into filtered sea-water. The effect of the lack of statoliths on the equilibrium of the animals can be thus observed. Within two hours after moulting, most of them swim about actively, and eat greedily when fed with bits of crab's liver. In swimming, however, they show quite marked abnormality. "There is both rolling from side to side, and 'pitching' forward and backward; often they swim with the ventral side uppermost. Much more easily overturned than normal larvae, they do not right themselves at once, but if turned upon the back, will continue to swim in that abnormal position. If blinded, the loss of equilibrium is still more marked. All these conditions are in strong contrast to the actions of the normal free-swimming larvae of these stages, which conduct themselves in the characteristic manner" (39, p. 238). These abnormalities, the result of the loss of equilibration, disappear at once when the larvae are allowed to obtain statoliths (39, p. 239). There is, therefore, no other explanation than that the statoliths, which are acted upon by the pull of gravity, maintain equilibrium in the line of the direction of gravity. This case is very important, because when we come to consider the geotropism of the higher vertebrates on which very extensive experiments have been made, we shall see that vertebrates deprived of their statoliths show the same abnormal phenomena manifested by lobster larvae without statoliths. This must be borne in mind.

III. Gravity, however, is not the only force which affects the behavior of animals. Many forces—light, contact stimulus, chemicals, temperature, and so forth, may act together. This leads to quite intricate results. Marine snails, for instance, which have statoliths, are naturally negatively geotropic.<sup>6</sup> But they are also negative to light. As affected by gravity, therefore, the animals are forced to crawl upward, but when exposed to light, they have to crawl downward. If the effective “strength” of gravity which depends on the position of the animals, is exactly equal to that of light, the animals might be stationary as a resultant of the two forces. If this is not the case, however, the stronger force might overcome the other. Gravity, of course, is constant—the pull of about 980 dynes—and always is exerted vertically. But the effective force exerted on the animals depends upon the position of the surface on which the animals may crawl. The exertion required to enable the animals to move on a horizontal surface is less than that required on a vertical surface, where the maximum force must be exerted. On the other hand, the intensity of light naturally changes, and can be artificially changed.

Suppose strong sunlight is falling vertically down, and the surface, on which the animals are crawling, is situated vertically. What direction must the animals take? The results of my experiments indicate that many of them—say 75%—crawl down. The light stimulus seems thus to be stronger than that of gravity. If you cover the vertical glass on which the snails are crawling down in direct sunlight with a dark box, you will be surprised to find the snails crawl upward in a few seconds; then, if you expose them to direct sunlight again, they will crawl down in a few seconds. They thus change their “minds” very rapidly!

Suppose again, we conduct an experiment in a room where there is no direct sunlight and place a vertical surface for the animals to move on. What must the animals do this time? Nearly all, if not all, crawl upward. Here is a problem for the psychologist, that is, a problem of “attention.” Is that “attention” which causes an animal to go down or up, when it is stimulated by the relatively stronger of two stimuli focused by an idea or train of ideas? Other things being equal, is it true that the greater the intensity of an experience, the greater its clearness? Or, is “attention” something directed by physical and chemical changes which are

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<sup>6</sup> A paper, “Studies on the Geotropism of the Marine Snail, *Littorina Littorea*,” is ready for publication.

produced by the relative intensity of light or gravity in the protoplasm of the cells, especially in a photosensitive or geosensitive region?

This point will soon become clear if we consider the reversibility of geotropism and heliotropism in the larvae of the marine annelid, *Arenicola cristata*. The larvae are naturally negative to gravity and positive to light. If you add 25 cc. of isotonic  $MgCl_2$  or  $CaCl_2$  solution to 25 cc. of sea-water, and transfer *Arenicola* larvae in this mixture, about two minutes later they all become positive to gravity and swim downward, being oriented with their anterior ends in that direction, though their positive heliotropism seems not to be changed very much. Instead of  $MgCl_2$  or  $CaCl_2$ , if you add 25 cc. of isotonic  $NaCl$  or weak acids to 25 or 50 cc. of sea-water, and transfer the larvae to this mixture, you will notice that the larvae become negative to light, though they, also, remain negative to gravity (24).

We may conclude that the change of the chemical composition, consequently of physical property, of the surrounding medium changes the geotropism and heliotropism of the larvae, although we do not yet know what change occurs in the organisms themselves.

Let us take another example to illustrate this point. We have already seen that *Arenicola* larvae become positive to gravity on addition of isotonic  $MgCl_2$  or  $CaCl_2$  solution to sea-water, though their light reaction is not affected very much. On the other hand, the larvae become negative to light on addition of isotonic  $NaCl$  solution to sea-water, though they remain negative to gravity. Now, it is well known among physiologists that  $CaCl_2$  or  $MgCl_2$  antagonizes the action of  $NaCl$  on organisms. In accord with this, if you add 5 cc. of isotonic  $CaCl_2$  solution to the mixture of 25 cc. of isotonic  $NaCl$  and 25 cc. of sea-water, you will find that the reversibility of positive heliotropism of the larvae is very much retarded. What does this mean? This seems to mean that mutual antagonism of  $CaCl_2$  and  $NaCl$  neutralizes the specific effect of each salt upon the larvae. This is physiologically of great significance. We seem therefore to be driven to the conclusion that both geotropism and heliotropism in animals are physico-chemical phenomena, and even such a problem as that of "attention," or the "initiative" of attention, as Royce expresses it, might be attacked by physico-chemical means instead of by entangled terms of consciousness (Cf. 10, 22, 41 and 48).

IV. It is well known that many insects show negative geotropism, but we cannot dwell on it here. So let us briefly

consider geotropism in still higher animals—the vertebrates, including mammals and man. Since Flourens experimentally showed in 1824 that extirpation of the vestibule and semi-circular canals caused marked disorders of equilibration, immense energy has been spent by physiologists upon the problem of a sense of equilibrium in fishes, birds, rats, dogs, cats, etc. In normal animals the statoliths which are analogous to those of lower animals are present in the utricle and saccule. That the incidence of the pressure of the statoliths on the hairs will vary according to the position of the animal, so that any change in the position of the head will be at once attended by alteration in the nerve fibres which have been stimulated by the pressure of the statoliths (Cf. 46, pp. 449-450), and therefore in the nature of the impulses regulating the locomotion and the maintenance of the equilibrium of the animal (a theory agreed to by most, but not all, investigators) has been already considered in reference to the “iron statoliths” of *Palamonetes*, to attract which a strong electro-magnet is employed.

On the other hand, the animals—fishes, birds, or dogs—which are deprived of the statoliths, or in which the semi-circular canals have been destroyed, show inability to co-ordinate the muscles used in standing, locomotion, or flying. “The character and extent of these results vary with the number of canals injured, and indeed, show a more or less definite relationship to the several canals. When the horizontal canal is cut on one side in pigeons, the animal makes movements of the head in the plane of that canal, and if the similar canal on the other side is also sectioned, these movements are more pronounced.” “When all three canals are cut on one or both sides, the animal shows a distressing inability to maintain a normal position. The head is twisted, it is not able to stand unless supported, and any attempt at walking or flying results in violent forced and incoördinated movements. The animal makes continued somersaults at each attempt to stand or walk, and the head is kept in spasmodic, forceful movements, which may produce injury or death.” “It should be added that results of this character are obtained only when the membranous canals are injured. If the bony canal alone is cut, and even if the perilymph is removed by suction, no such effects are obtained” (21, pp. 399-400). These descriptions remind us of the observations on lobster larvae, already referred to, which were kept without statoliths.

In man, as in the shrimp, it is the statolith organ which determines the behavior in relation to the force of gravity. The statolith organ is therefore responsible in part at least,

not only for the maintenance of equilibrium, but also for the sensations which enable a man to orient himself and to know the position in which he happens to be at any given moment. There has been, of course, no experimentation on man deprived of the statoliths, or with the semicircular canals destroyed. But clinical data (1 and 42) point to the same conclusion, and cats and dogs have served for further proof (43).

V. As indicated briefly above, the facts regarding injury to and stimulation of the semicircular canals and the statolith organs, are very numerous, and, on the whole, fairly concordant. Their interpretation, however, has offered great difficulties, and many views have been proposed. Almost every investigator, in fact, has, to some extent, varied in his interpretation of the precise functional significance of these organs. We can, therefore, understand why there are many theories concerning them.

But a consideration of the structure of the statolith organs<sup>7</sup> shows at once that the incidence of the weight of the statoliths on the hairs of the maculae in the utricle and saccule will vary according to the position of the head, or of the animal's body. The nerve-endings stimulated by the weight of the statoliths will therefore vary according to the position of the head, or of the animal. The impulses of the stimulated nerve-endings will flow to the central nervous system. A reaction of the animal will obviously be the result (46, p. 680-681).<sup>8</sup>

When we have thus considered, though very briefly, geotropism in animals from the unicellular organism to man, we can fairly understand what an important rôle as stimulus the force of gravity plays in the physiology of animals in general. Consciousness, on the other hand, seems to play little part in the orientation of animals to gravity.

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<sup>7</sup> The semicircular canals must very probably be a part of the organ of static equilibrium. The eyes also have an important rôle in equilibration, but the writer has purposely omitted consideration of it in this paper.

<sup>8</sup> It seems to us therefore that the theory of Mach is of great significance.



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## A COMPARISON OF REFLEX THRESHOLDS WITH SENSORY THRESHOLDS—THE RELATION OF THIS COMPARISON TO THE PROBLEM OF AT- TENTION

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In a series of papers from this laboratory<sup>1</sup> variations in the sensory threshold for faradic stimulation in man under various conditions have been described. The assumption has been made throughout the series that these variations in threshold result largely from changes in the central nervous system, and that they may be used, therefore as indicators of central nervous changes. The chief basis for this assumption is the observation that the diurnal variations in sensory threshold parallel closely variations in voluntary muscular activity as determined by ergographic fatigue.<sup>2</sup> Although no theoretical considerations nor any experimental data incompatible with this assumption have come to our attention we have thought it worth while to carry on an investigation designed expressly to test the validity of the use of the sensory threshold as an index of the general state of the nervous system.

The test consisted of repeated comparisons, extending over a considerable period, of the threshold of an easily evoked reflex, the winking reflex, from faradic stimulation of a selected spot on the lower lid, with the sensory threshold for the same spot on the lid. In this comparison the receptors and the afferent pathways as far as the central nervous system are presumably the same for both tests. If our present understanding of the functioning of the nervous system is correct the pathways within the central nervous system must be quite different. That for completion of the reflex would be pictured as a chain of neurones within the brain stem, leading ultimately into the efferent neurones for the muscle

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<sup>1</sup> Grabfield and Martin: *American Journal of Physiology*, XXXI, 1913, 300; Martin, Bigelow, and Wilbur: *Idem.*, XXXIII, 1914, 415; Martin, Withington, and Putnam: *Idem.*, XXXIV, 1914, 97.

<sup>2</sup> Grabfield and Martin: *Loc. cit.*, 308.

of winking; the path for the sensory perception, on the other hand, would be traced into the cortex of the cerebrum.

One would naturally suppose that the cerebral cortex, since it is the most highly organized portion of the nervous system, should also be the most variable. If the sensory threshold is a valid indicator of cortical condition it would be expected to show more pronounced fluctuations from time to time than would the threshold for such an activity as the reflex, which presumably involves less unstable nervous elements.

*Method.* For applying faradic stimuli one "indifferent" electrode and one stimulating electrode were used. The "indifferent" electrode consisted of a small brass plate about which several layers of absorbent gauze were wrapped. The electrode, well moistened with physiological saline, was applied to the forehead and secured in place by means of bandages about the head. A satisfactory stimulating electrode was secured only after much experimenting with various designs. The one finally used, which gave excellent results, consisted of a short piece of rubber tubing, 7 mm. inside diameter, passed through a hole in a common cork so that about 3 mm. projected beyond the end of the cork. Into the opposite end of the rubber tube from that which projected was inserted an amalgamated metal bar (copper at first, later, for greater durability, zinc). This bar bore a small binding post. It was secured in the tube with sealing wax. The tube next the metal bar was filled with calomel paste. On top of this, and filling the projecting tip of the tube flush to its edge, was a paste of kaolin with salt solution. When the tube filled with this paste was placed in contact with the spot on the lower lid which had been determined experimentally as the best for evoking the wink reflex, the cork rested against the cheekbone, and a simple bandage around the head held it securely in place.

Thresholds were determined quantitatively according to the method developed by one of us.<sup>3</sup> Through the kindness of Drs. Southard and Adler, of the psychopathic department of the Boston State Hospital, we were permitted to use the apparatus belonging to that institution for our observations. We take this opportunity of expressing our appreciation of their generosity.

For the routine work of the Psychopathic hospital the complete determination of sensory thresholds in terms of  $\beta$  units<sup>4</sup> has been found to involve too great a strain upon the

<sup>3</sup> Martin: The measurement of induction shocks. New York, 1912.

<sup>4</sup> Martin: Loc. cit., 76.

patients. For that reason the determination of tissue resistance in each subject is not made, and at the time we were carrying on this investigation the equipment for making resistance measurements was lacking. Our results are in terms of Z units,<sup>5</sup> therefore, rather than in terms of the more exact  $\beta$  units. For the particular purpose of this study Z units suffice fully, as we shall show later, although there would have been an advantage in being able to express our data in  $\beta$  units, chiefly to make them more readily comparable with the data of other investigations.

*Observations.* In this work two medical students, P. and W., young men in good health, acted alternately as subjects and observers. Sixty-six readings were made upon W. and 65 upon P. The observations were commenced March 29, 1914, and terminated April 30, 1914. No set time of day was selected for taking readings. On the contrary, it was thought desirable to obtain them at irregular intervals. On some days four readings were made on each subject; on others only one. Two hours was the smallest interval between readings on the same subject.

Save that the region of stimulation was the lower lid instead of the finger tips and that tissue resistances were not measured, the determination of sensory thresholds was according to the method of Grabfield and Martin.<sup>6</sup> Promptly upon the completion of the sensory determination the subject seated himself directly in front of the observer with his eyes well illuminated, and the threshold of the winking reflex was established. As a matter of fact, the attempt was made to establish two such thresholds, one for the least perceptible movement of the eyelid, and the other for complete closure of the eye. The latter of these was found to be more accurately determinable, and was finally adopted, therefore, as the reflex threshold to be used. Care was taken to avoid errors through mistaking spontaneous winks for those resulting from the test stimuli.

*Results.* In both subjects the sensory threshold was markedly lower than either of the two reflex thresholds studied. For subject P. the average values of Z from 65 experiments were; sensory threshold, 405; threshold of least perceptible lid movement, 578; threshold of complete closure of eye, 816. For subject W. the average values of Z from 66 experiments were: sensory threshold, 504; threshold of least

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<sup>5</sup> Martin: Loc. cit., 73.

<sup>6</sup> Grabfield and Martin: Loc. cit., 303.

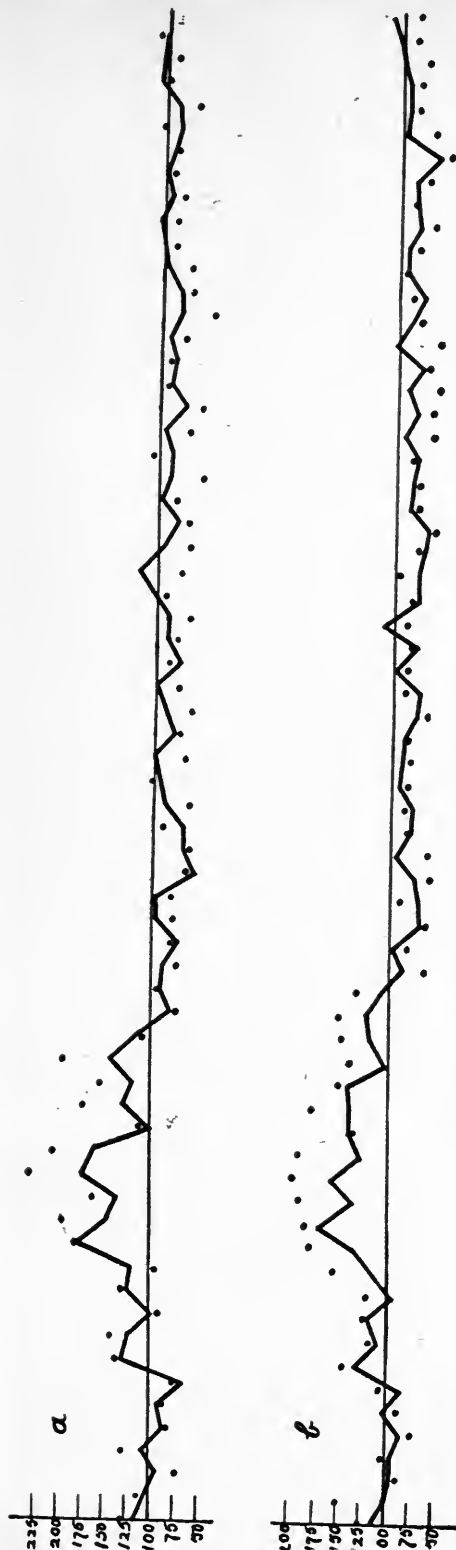


Figure 1. Sensory thresholds compared with reflex thresholds: a, subject P; b, subject W. The thresholds are expressed in terms of percentile divergence from the mean. In each figure a horizontal line is drawn at the level of the mean threshold. The data are set down in chronological sequence. The irregular solid line connects the successive values of the reflex threshold. The dots represent the corresponding sensory thresholds.

perceptible lid movement, 690; threshold of complete eye closure, 990. As one of us has shown elsewhere,<sup>7</sup> Z units express stimulation strengths only approximately, since they take no account of tissue resistance nor of the manner of electrode contact, although where the general experimental conditions are kept as constant as they were throughout this investigation, the error is not likely to be very large.<sup>8</sup> However, *relative values* can be accurately stated in Z units so long as secondary resistance and electrode contact remain constant. These conditions were fulfilled in individual experiments of this series. The relation of reflex threshold to sensory threshold *in any given experiment* is therefore accurately known, within the limits of observational error.

If the mean of all values of Z for the sensory threshold of a single subject is determined, and this mean is compared with the value of Z for any single determination, the extent of departure of the individual Z from the mean depends in part upon the fact that the Z unit does not express with entire accuracy the true value of the stimulus, and in part upon an actual difference in threshold of this particular reading as compared with the mean. The same factors enter to account for divergences of individual reflex thresholds from their mean. However, in any given experiment, the extent of divergence, which enters through error in the accuracy of the Z units as expressions of stimulation strengths, must be exactly the same for both sensory and reflex tests. Therefore, if, in any experiment, these observed thresholds show different degrees of divergence from their respective means they are actually as well as apparently, unequally divergent, and the one which, in terms of Z units, appears to be the more divergent, is so actually. Whatever conclusions may legitimately be drawn from study of the relative divergences of the two sorts of thresholds from their respective means are therefore justified.

For convenience and ease of comparison our results are presented in terms of percentages. The method of working these out is that used by Grabfield and Martin.<sup>9</sup> We have six sets of data; a series of sensory thresholds; a series of thresholds for least perceptible reflex lid movement, and a series of thresholds for complete reflex closure of the eye for each subject. For each set the mean was determined. These mean values are given above, pp. 430ff. The individual

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<sup>7</sup> Martin: *American Journal of Physiology*, XXVII, 1910, 228.

<sup>8</sup> Martin: *Ibid.*, 232.

<sup>9</sup> Grabfield and Martin: *Loc. cit.*, 307.



thresholds were reduced to percentage terms by means of the formula: mean threshold; 100 = individual threshold:  $\times$ . By this scheme reflex and sensory thresholds become directly comparable. To bring out the points we wish to make the thresholds for complete reflex closure of the eye are compared with the sensory thresholds. The data from the thresholds for least perceptible lid movement are confirmatory of our conclusions, but the experimental determinations seemed to us less certain, and their inclusion seems not to be essential. Figure 1, a and b, gives the results of our two series, expressed in per cents. In both our subjects the thresholds for the reflex show on the whole markedly less divergence from the mean than do the sensory thresholds. The percentage divergence of any individual reading is simply determined by subtracting its "reduced" value from 100 if it is below the mean, or subtracting 100 from its "reduced" value if it is above. The average of all the percentage divergences thus determined for a single series of thresholds serves as an expression for the "tendency to diverge" of the members of that series. Expressed thus our results are: for reflex thresholds, subject P. 17.3, subject W. 20; for sensory thresholds, subject P. 28.6; subject W. 31. Detailed analysis of our data brings out even more strikingly than do the above averages the greater "tendency to diverge" of sensory as compared with reflex thresholds. Thus in subject P. in 50 out of 65 experiments, and in subject W. in 47 out of 66 experiments the reflex thresholds are nearer the mean than are the corresponding sensory thresholds. The reflex thresholds group themselves, on the whole, within a much narrower range than do the sensory. Thus the range of "reduced" thresholds included between values 75 and 125 contains 77 per cent of the reflex thresholds for P. and 71 per cent for W.; whereas the same range includes only 57 per cent of the sensory thresholds for P. and 50 per cent for W. Of individual thresholds varying from the mean more than 50 per cent the reflex series show only 5, 3 for P, and 2 for W. The sensory series, on the other hand, include 19 such wide divergences, 9 for P. and 10 for W.

The summaries just presented are based upon our entire series of experiments. Reference to the figure shows that through a period of the investigation the thresholds, reflex and sensory, for both subjects were considerably higher than during the remainder of the time. This period of high thresholds began on April third and ended on April tenth. So far as we are aware there was no change in the procedure during

this period, yet various considerations incline us to the belief that an unnoticed variation either in the preparation of the electrodes, their application to the body surface, or the direction of current through them, was the source of the variation in thresholds. The change in thresholds during this period was not duplicated in four other subjects who were making sensory tests with another apparatus at the same time that this investigation was in progress. That the electrodes, and not other parts of the apparatus, were responsible is indicated by the circumstance that on April sixth, in the middle of this period, a set of readings was made on each subject with the apparatus in this laboratory, but with the usual electrodes, brought from the psychopathic hospital for the purpose, and in these readings the high thresholds, characteristic of the period, were recorded.

As we believed the unusual character of these thresholds to be clearly instrumental, we felt justified in including them in our series, since any variations dependent on changes in the apparatus should affect sensory and reflex thresholds alike. To assure ourselves, however, that we were not in error in thus including these unusual thresholds, we have re-examined the two series with the period of high thresholds omitted. The series as thus amended show decidedly lower mean thresholds than those given above (pp. 430ff.). For subject P. the values are: sensory threshold, 51 experiments, 343; reflex threshold 735; as compared with 405 and 816 respectively, for the entire series of 65 tests. For subject W. the mean values from 49 experiments are: sensory threshold, 409, reflex threshold 864, as compared with 504 and 990, from the entire series.

The "tendency to diverge" from the mean is markedly less in magnitude in the amended series, but is still definitely greater for the sensory thresholds than for the reflex thresholds. The figures are: for subject P. reflex thresholds 10.8, sensory thresholds 14; for subject W. reflex thresholds 11.4, sensory thresholds 15.1. In maximal extent of divergence and in number of wide divergences, the sensory thresholds also continue to lead. The greatest reflex divergences are: subject P., 34 per cent, subject W., 36 per cent, as compared with sensory divergences of 52 and 83 per cent, respectively. The range of "reduced" thresholds between values 75 and 125 includes all but 8 per cent of the reflex thresholds for subject P., whereas 18 per cent of his sensory thresholds are without this range. For subject W. the corresponding figures are 6 per cent and 16 per cent respectively.

These results show clearly that in our subjects the threshold for faradic elicitation of the winking reflex is, on the whole, definitely steadier over a considerable period of time than is the electro-cutaneous sensory threshold. Are we justified in interpreting these results as supporting the conclusion that the sensory threshold is a reliable indicator of the general state of the higher nerve centers? An objection which may be urged against such an interpretation, and, in fact, has been so urged by a psychologist with whom the matter has been discussed, is that the fluctuations in sensory threshold may have been due chiefly to variations in the attention of the subject at the different periods of experimentation. Obviously variations in attention cannot be excluded in experiments of this sort. In fact, sensory threshold tests are of necessity in a sense, tests of attention. All that can be done is to instruct the subject to attend as strictly as possible to his part of the procedure. Strong features of this test are that it can be carried out very quickly and that the procedure required of the subject is of the simplest character, so simple, in fact, that the test has been applied successfully to numerous psychopathic cases.<sup>10</sup> The concentration of attention is not rendered difficult by the necessity of following complicated directions, nor is any marked feeling of strain involved. Our experience is that subjects, upon becoming accustomed to the routine, fall into a mental state during the course of individual experiments which seems to be about the same from experiment to experiment. Many psychologists are of the opinion that attention is ordinarily less fluctuating than has formerly been supposed.<sup>11</sup> Geissler<sup>12</sup> has shown that fluctuations in the time required for making mental additions agree strikingly with introspective estimates of degree of attention. In his opinion we are entitled, therefore, to base judgments of attention on observed addition times. He presents data from 12 series of experiments without distraction, made on 3 subjects (*loc. cit.*, table II). The greatest fluctuations from average addition time do not exceed 31 per cent, and by far the greater number amount to less than 15 per cent. He found, moreover (*loc. cit.* p. 513), that complex schemes of distraction did not induce great variations in attention. According to these results the variations of sensory threshold due to fluctuations of attention would be expected to be much smaller than those obtained by us in this investigation, and

<sup>10</sup> Grabfield: Boston medical and surgical journal, CLXXI, 1914, 883.

<sup>11</sup> See Titchener: Textbook of Psychology. New York, 1910. 291.

<sup>12</sup> Geissler: This Journal, XX, 1909, 508.

we feel that except in so far as the attention is a function of the general nervous state it probably does not enter as a controlling factor in determinations of sensory thresholds by this method.

Certain data from these experiments appear to have a bearing upon the general problem of the fluctuation of attention. In the first place each individual sensory test contains within itself the means of determining whether during its course the attention has fluctuated to a serious extent. This means is afforded through the necessity of determining several thresholds in order that the value of  $\beta$  may be calculated.<sup>13</sup> If the several calculated  $\beta$ s fail to agree within a reasonable margin (ten per cent is the limit we allow<sup>14</sup>), the failure to agree is probably due to a fluctuation of attention. That fluctuations of attention would ever so relate themselves as to conceal their existence is extremely improbable. Determinations have been made in great numbers in this laboratory during recent years, and very seldom, in the hands of experienced workers, must experiments be rejected on account of failure of the calculated  $\beta$ s to agree within ten per cent.

In this particular investigation  $\beta$  units could not be determined because facilities for measuring secondary resistances were lacking, but in each experiment the routine procedure of establishing the usual series of four thresholds was followed, so that the course of the attention during individual experiments could be determined. As a matter of fact the agreement among the different threshold tests of individual experiments is as close in this investigation as in any others that have been carried on in this laboratory, showing that during the period of single experiments (about five minutes) important attention fluctuations have not usually occurred.

Of significance in connection with the problem of the fluctuation of attention from hour to hour and from day to day is the observation that the *direction of divergence* of individual thresholds from the mean was nearly always the same, in any single experiment, for both reflex and sensory thresholds. The two thresholds rose or fell together, but the sensory rose or fell further than the reflex. In only 10 of the 131 experiments were reflex and sensory thresholds on opposite sides of their respective means. In 92 of the 121 experiments in which they were on the same side of the mean the sensory thresholds were further from their means than were the reflex thresholds from theirs.

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<sup>13</sup> Martin: The measurement of induction shocks, 77.

<sup>14</sup> Martin: American Journal of Physiology, XXXVI, 1915, 224.

The fluctuations from their means of the *reflex* thresholds may be taken as a base line. If the sensory thresholds had been found to fluctuate in exact harmony with the reflex thresholds we should have been obliged to conclude that there are no variations in higher brain centers that are not duplicated in lower centers. Had the sensory thresholds fluctuated irregularly about the line traced by the reflex thresholds a simple deduction would have been that variations of attention in the ordinary sense, were responsible. But when the sensory thresholds are seen to follow preponderatingly a course of greater deviation than do the reflex thresholds we feel justified in concluding that something more deep seated is indicated, and that we probably are dealing directly with attention as a function of the condition of the higher brain centers.

#### SUMMARY

Comparisons in a long series of experiments of threshold stimuli for evoking the winking reflex with sensory threshold stimuli show that the former tend to vary from their mean value definitely less than do the latter.

The observation is made that the "direction of divergence" is nearly always the same for sensory as for reflex thresholds. This is taken to mean that the attention, under the conditions of these experiments, manifests itself as a function of the condition of the higher brain centers.

We draw the general conclusion that the value of the sensory threshold depends on the state of the higher nerve centers, and that the threshold may justly be used, therefore, as an indicator of such nervous state.

## THE FREUDIAN METHODS APPLIED TO ANGER

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By G. STANLEY HALL<sup>1</sup>

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The exact sciences consist of a body of truth which all accept, and to which all experts strive to contribute. Philosophy, however, like religion, has always been broken into sects, schools or parties, and the body of truth which all accept in these fields is relatively far less, and the antagonistic views far greater. Normal psychology, which a few decades ago started out to be scientific with the good old ideal of a body of truth *semper ubique et ad omnibus*, is already splitting into introspectionistic, behavioristic, genetic, philosophical and other groups, while in the new Freudian movement, Adler and Jung are becoming sectaries, the former drawing upon himself the most impolitic and almost vituperative condemnation of the father of psychoanalysis. With this latter schism we are not here concerned, but we are deeply concerned with the more general relations between the psychologists of the normal and those of the abnormal. With a very few negligible exceptions psychoanalysis has never had a place on the program of our American Psychological Association, and the normal has had little representation in your meetings and publications. This I deem unfortunate for both, for unsatisfactory as this sadly needed *rapprochement* is on the continent, it is far more so here. That the normalists in this country so persistently ignore the unique opportunity to extend their purview into the psychopathological domain at the unique psychological moment that the development of Freudianism has offered, is to me a matter of sad disappointment and almost depression. In reading a plea for Freud in our association of normalists, I am a *vox clamantis in deserto* and can evoke no response, and even the incursions of psychoanalysis into the domain of biography, myth, religion and dreams, have not evoked a single attempt at appreciation or criticism worthy of mention by any American psychologist of the normal. I have sought in various

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<sup>1</sup> Read at a meeting of the American Psychoanalytical Association, New York City, May 5, 1915.

ways the causes of this reticence, not to say ignorance. While I received various answers, the chief one was to the effect that the alleged hypertrophy of sex in its gross pathological forms, and the conviction of the kind and degree of sex consciousness found in the many hundreds of analyzed cases, constitute the very essence of the neurotic and psychotic cases, but that conscious and unconscious sex factors are slight or absent in most normal cases; that these patients and their doctors alike are sex-intoxicated, and that the Freudian psychology applies only to perverts and erotomaniacs or other abnormal cases. To ascribe all this aversion on their part to social and ethical repression is both shallow and banousic, for the real causes are both manifold and deeper. They are part of a complicated protest of normality, found in all and even in the resistance of subjects of analysis, which is really a factor basal for self-control, of the varying good sides of which Freudians tell us nothing. The fact is that there are other things in the human psyche than sex and its ramifications. Hunger, despite Jung, fear despite Sadger, and anger despite Freud, are just as primary, aboriginal and independent as sex, and we fly in the face of fact and psychic experience to derive them all from sex, although it is freely granted that in morbid cases each may take on predominant sex features. I think a glance will show how the Freudian mechanisms apply quite as well to anger as to sex.

(1) Anger in most of its forms is the most dynamogenic of all the emotions. In paroxysms of rage with abandon we stop at nothing short of death and even mutilation of the corpse. The Malay running amuck, Orlando Furioso, the epic of the wrath of Achilles, hell-fire, which is an expression of divine wrath, are some illustrations of its power. Savages work themselves into frenzied rage in order to fight their enemies. In many descriptions of its brutal aspects, which I have collected, children and older human brutes spit, hiss, yell, snarl, bite noses and ears, scratch, gouge out eyes, pull hair, mutilate sex organs, with a violence that sometimes takes on epileptic features and which in a number of recorded cases causes sudden death at its acme, from the strain it imposes upon the system. Its cause is always some form of thwarting wish or will or of reduction of self-feeling, as anger is the acme of self-assertion. The German criminalist, Friedrich, says that probably every man might be caused to commit murder if provocation were sufficient, and that those of us who have never committed this crime owe it to circumstances and not to superior power of inhibition. Of course it may

be associated with sex but probably no human experience is *per se* more diametrically opposite to sex.

Some temperaments seem to crave, if not need, outbreaks of it at certain intervals, like a certain well-poised lady, so sweet-tempered that everybody imposed on her, till one day at the age of twenty-three she had her first ebullition of temper and went about to her college mates telling them plainly what she thought of them. She went home rested and happy, full of the peace that passeth understanding. Otto Heinze, and by implication Pfister, think nations that have too long or too assiduously cultivated peace must inevitably, sooner or later, relapse to the barbarisms of war to vent their instincts for combat, and Crile thinks anger most sthenic, while Cannon says it is the emotion into which most others tend to pass.

It has, of course, been a mighty agent in evolution, for those who can summate all their energies in attack have survived. But few if any impulsions of man, certainly not sex, have suffered more intense, prolonged or manifold repressions. Courts and law have taken vengeance into their hands or tried to, and not only a large proportion of assaults, but other crimes, are still due to explosions of temper, and it may be a factor in nearly every court case. Society frowns on it, and Lord Chesterfield says the one sure and unfailing mark of a gentleman is that he never shows temper. Its manifestations are severely tabooed in home and school. Religion teaches us not to let the sun go down upon our wrath and even to turn the other cheek, so that we go through life chronically afraid that we shall break out, let ourselves go, or get thoroughly mad, so that the moment we begin to feel a rising tide of indignation or resentment (in the nomenclature of which our language is so very rich, Chamberlain having collected scores of English expressions of it), the censorship begins to check it. In many cases in our returns repression is so potent from long practice, that the sweetest smile, the kindest remarks or even deeds are used either to veil it to others, or to evict it from our own consciousness, or else as a self-inflicted penance for feeling it, while in some tender consciences its checked but persistent vestiges may become centers of morbid complexes and in yet other cases it burrows and proliferates more or less unconsciously, and finds secret and circuitous ways of indulgence which only psychoanalysis or a moral or religious confessional could trace.

(2) Anger has many modes of *Verschiebung*, both instinctive and cultivated. One case in our returns carries a bit of wood in his vest pocket and bites it when he begins to



feel the aura of temper. Girls often play the piano loudly, and some think best of all. One plays a particular piece to divert anger, viz., the "Devil's Sonata." A man goes down cellar and saws wood, which he keeps for such occasions. A boy pounds a resonant eavespout. One throws a heavy stone against a white rock. Many go off by themselves and indulge in the luxury of expressions they want none to hear. Others take out their tantrum on the dog or cat or perhaps a younger child, or imprecate some absent enemy, while others curse. A few wound themselves, and so on, till it almost seems, in view of this long list of vicariates, as if almost any attack, psychic or physical, might thus be intensified, and almost anything or person be made the object of passion. Be it remembered, too, that not a few look, do, think, feel their best under this impulsion.

(3) Besides these modes of *Abreagierung* there are countless forms of sublimation. In anger a boy says: I will avenge myself on the bully who whipped me and whom I cannot or will not whip, by besting him in his studies, class-work, composition, or learn skillful stunts that he cannot do, dress, or behave better, use better language, keep better company, and thus find my triumph and revenge. A man rejected or scorned by a woman sometimes makes a great man of himself, with the motivation more or less developed to make her sorry or humiliated. Anger may prompt a man to go in to win his enemy's girl. A taunt or an insult sometimes spurs the victim of it to towering ambition to show the world and especially the abuser better, and to be able to despise him in return; and there are those who have been thus stung to attempt greatness and who find the sweetest joy of success in the feeling that by attaining it they compensate for indignities they suffered in youth.

In fact, when we analyze ambition and the horror of *Minderwertigkeit* that goes with it, we shall doubtless find this factor is never entirely absent, while if we were to apply the same pertinacity and subtlety that Jung in his "*Wandlungen*" has brought to bear in working over the treacherous material of mythology, we might prove with no less verisimilitude than he has shown the primacy of the libido that in the beginning was anger, and that not Anaxagoras' love or the strife of Heraclitus was the *fons et origo* of all things, that the *Ichtrieb* is basal, and that the fondest and most comprehensive of all motives is that to excel others, not merely to survive, but to win a larger place in the sun, and that there is some con-

nection between the Darwinian psychogenesis and Max Stirner and Nietzsche, which Adler has best evaluated.

(4) Anger has also its dreams and reveries. When wronged the imagination riots in the fancied humiliation and even tortures of an enemy. An object of hate may be put through almost every conceivable series of degradation, ridicule, exposure and disgrace. He is seen by others for what our hate deems him to be. All disguises are stripped off. Children sometimes fancy a hated object of anger flogged until he is raw, abandoned by all his friends, an outcast, homeless, alone, in the dark, starving, exposed to wild animals, and far more often more prosaic fancies conceive him as whipped by a parent or stronger friend, or by the victim himself later. Very clever strategies are thought out in detail by which the weaker gets even with or vanquishes the stronger, and one who suffers a rankling sense of injustice can hardly help day-dreaming of some form of come-uppance for his foe, although it takes years to do it. In these reveries the injurer in the end almost always gives up and sues for mercy at the feet of his quondam victim. So weird and dramatic are these scenes often that to some minds we must call anger and hate the chief springs of the imagination. A pubescent girl who was deeply offended went off by herself and held an imaginary funeral of her enemy, hearing in fancy the disparaging remarks of the bystanders, and when it was all over and the reaction came, she made up with the object of her passion by being unusually sweet to her and even became solicitous about her health as fearing that her revery might come true. We all too remember Tolstoi's reminiscences when, having been flogged by his tutor, he slunk off to the attic, weeping and broken-hearted, and finally after a long brooding resolved to run away and become a soldier, and this he did in fancy, becoming corporal, lieutenant, captain, colonel. Finally came a great battle where he led a desperate charge that was crowned with victory, and when all was over and he stood tottering, leaning on his sword, bloody and with many a wound, and the great Czar of all the Russias approached, saluted him as saviour of his fatherland and told him to ask whatever he wanted and it was his, he replied magnanimously that he had only done his duty and wanted no reward. All he asked was that his tutor might be brought up and his head cut off. Then the scene changed to other situations, each very different, florid with details, but motivated by ending in the discomfiture of the tutor. In the ebb or

ambivalent reaction of this passion he and the tutor got on better.

Richardson has collected 882 cases of mild anger, introspected by graduate students of psychology, and finds not only over-determination, anger fetishes and occasionally anger in dreams with patent and latent aspects and about all the Freudian mechanisms, but what is more important, finds that very much of the impulsion that makes us work and strive, attack and solve problems has an element of anger at its root. Life is a battle and for every real conquest man has had to summate and focus all his energies, so that anger is the acme of the manifestation of Schopenhauer's will to live, achieve and excel. Hiram Stanley rather absurdly described it as an epoch when primitive man first became angry and fought, overcoming the great quaternary carnivora and made himself the lord of creation. Plato said anger was the basis of the state, Ribot made it the establisher of justice in the world, and Bergson thinks society rests on anger at vice and crime, while Stekel thinks that temper qualities should henceforth be treated in every biography and explored in every case that is psychoanalyzed. Hill experimented with pugilism, and Cannon pleads for athletics as a legitimate surrogate for war in place of James' moral substitute. Frank Howard's opinion is that an impulse that Darwin finds as early as the sixth week and hardly any student of childhood later than the sixth month, should not be repressed but developed to its uttermost, although carefully directed to worthy objects, and he pleads for judicious scolding and flogging, to be done in heat and not in cold blood, and says that there is enough anger in the world, were it only rightly directed, to sweep away all the evils in it. In all these phenomena there is no trace of sex or any of its symbols, and sadism can never explain but must be explained by it.

In this very slight paper I am only trying to make the single point which I think fear and sympathy or the gregarious or social instinct would still better illustrate, although it would require more time, that the movement inaugurated by Freud opens up a far larger field than that of sex. The unconscious that introspectionists deny, asserting that all phenomena ascribed to it are only plain neural mechanisms, and therefore outside the realm of psychology, the feelings which introspection can confessedly never tell much about and concerning which our text-books in psychology still say so little, are marking a new epoch in this field, and in that the chief merit of Freudianism consists.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER and H. P. WELD

XXIV. ON THE VARIATION WITH TEMPERATURE OF THE PITCH OF WHISTLES AND VARIATORS

By G. J. RICH

We recently had occasion to tune a set of Stern tone-variators by comparison with tuning-forks. When the comparison was repeated, a day or two later, the series proved to be slightly out of tune; and this occurred several times. It was natural to think that the change was due to change in the temperature of the room; and reference to works on physics showed that such a variation in pitch might be the result of variation in temperature. It is the purpose of this paper to show this dependence of pitch on temperature, and to set forth the precautions that must therefore be taken in calibrating a whistle or variator.

In the numerous articles which have dealt with the construction, use, and accuracy of these instruments, the influence of temperature has been neglected; and this in spite of the fact that it has been recognized in discussions of the pitch of organ pipes. Ellis<sup>1</sup> mentions that organ pipes change in pitch with temperature, and (as will be noted later) gives a formula for correction. Galton,<sup>2</sup> in describing his whistles, gives a formula for their pitch, which is, he says, valid for ordinary conditions of temperature. But only three of the later writers recognize that temperature-conditions are not always 'ordinary.' Stumpf and Meyer,<sup>3</sup> speaking of a Galton whistle blown by compressed air, say that the compression-apparatus must not be used for too long a time without interruption, because the temperature of the air-stream will change, and cause a slight change in the tone. Myers<sup>4</sup> records as an objection to the Stern variator its sensitivity to slight changes in temperature. But Appunn,<sup>5</sup> Stumpf,<sup>6</sup> Melde,<sup>7</sup> Schwendt,<sup>8</sup> Schulze,<sup>9</sup> Hegener,<sup>10</sup> and Jones,<sup>11</sup> in discussing the ac-

<sup>1</sup> Helmholtz, *Sensations of Tone*, 1895, 89, footnote.

<sup>2</sup> *Inquiries into Human Faculty*, 1883, 375.

<sup>3</sup> *Ann. d. Phys. u. Chem.* (Wiedemann), 61, 1897, 760; 64, 1898, 409.

<sup>4</sup> *Mind*, N. S., 20, 1911, 285.

<sup>5</sup> *Ann. d. Phys. u. Chem.* (Wied.), 67, 1899, 217.

<sup>6</sup> *Ibid.*, 68, 1899, 105.

<sup>7</sup> *Ibid.*, 67, 1899, 781.

<sup>8</sup> *Archiv für Ohrenheilkunde*, 49, 1900, 1; *Verhandlungen der Gesell. deutscher Naturf. u. Aerzte*, 71, 1900, 369; *Verhandl. d. deutsch. otologischen Gesell.*, 9, 1900, 55.

<sup>9</sup> *Ann. d. Phys.* (Drude), 13, 1904, 1066; 24, 1907, 784.

<sup>10</sup> *Verhandl. d. deutsch. otologischen Gesell.*, 17, 1908, 79; 72.

<sup>11</sup> *Edinburgh Medical Journal*, 11, 1902, 349.

curacy of Galton whistles, make no mention of temperature. In other papers, Schwendt,<sup>12</sup> Schulze,<sup>13</sup> Myers,<sup>14</sup> and Titchener,<sup>15</sup> using Kundt's dust-figures, Quincke's tubes, or sensitive flames in the determination of the pitch of whistles, have made corrections for temperature in the constants used to calculate frequency from the crude data, but have taken no account of a temperature-change in frequency itself. Edelman,<sup>16</sup> using Kundt's method, makes no correction for temperature, but takes as his constant that for average temperature. Stern,<sup>17</sup> in describing his variators, makes no mention of the dependence of pitch on temperature.

In a Galton whistle, or in any form of piston-whistle, as well as in a Stern variator, we have, of course, a vibrating body of air. For such a body, it follows from the theory of dimensions<sup>18</sup> that frequency is proportional to  $\frac{\sqrt{p/d}}{L}$ ,  $p$  being the pressure and  $d$  the density of air,  $\sqrt{p/d}$  the velocity of sound in air,<sup>19</sup> and  $L$  any length of the containing chamber. Inserting an arbitrary constant, we get for  $N$ , the frequency of our whistle:<sup>20</sup>

$$N = C \frac{v}{L},$$

where  $C$  is constant for all geometrically similar containing chambers.

But  $v$ , the velocity of sound in air, changes with temperature. Watson<sup>21</sup> gives the following formula for  $v_t$ , the velocity at temperature  $t$ , when  $v_0$ , the velocity at  $0^\circ$ , is known:

$$v_t = v_0 (1 + .0018t).$$

This gives us:

$$N = C \frac{v_0 (1 + .0018t)}{L}.$$

<sup>12</sup> *Arch. f. ges. Physiol.*, 71, 1899, 346; *Ver. d. naturf. Ges. in Basel*, 12, 1900, 149.

<sup>13</sup> *Ann. d. Phys. u. Chem. (Wied.)*, 68, 1899, 99; 869.

<sup>14</sup> *Jour. of Physiol.*, 28, 1902, 417.

<sup>15</sup> *Proc. Amer. Phil. Soc.*, 53, 1914, 328.

<sup>16</sup> *Ann. d. Phys. (Drude)*, 2, 1900, 469; *Zeit. f. Ohrenheilkunde*, 36, 1900, 330.

<sup>17</sup> *Zeit. f. Psychol.*, 11, 1896, 4; 21, 1899, 361; 30, 1902, 422; *Verhandl. d. physik. Gesell. su Berlin*, 16 Jahrgang (4), 1897, 42; *Psychol. der Veränderungsauffassung*, 1898, 82.

<sup>18</sup> This use of the theory of dimensions is justified by Rayleigh, who employs it for the frequency of strings. *Theory of Sound*, 1877, i, 139. For this method of deriving the formula I am indebted to Dr. J. Slepian of the Department of Mathematics, Cornell University.

<sup>19</sup> Watson, *A Text-Book of Physics*, 364.

<sup>20</sup> Since  $N$  is proportional to  $\frac{v}{L}$ , it is equal to a constant times this. The constant we have not determined. It depends on the shape of the containing chamber, and the direction in which the measurement  $L$  is taken.

<sup>21</sup> Watson, *op. cit.*, 367.

<sup>22</sup> This is, save for the constants, practically the formula given for the dependence of the pitch of organ-pipes on temperature. Barton, *Text-Book on Sound*, 252.

It remains to show that  $C$  and  $L$  do not change appreciably with temperature. When a whistle-pipe or variator-cylinder expands with temperature, it does so in all directions, and remains geometrically similar at all temperatures; and we have seen that  $C$  is constant under just these conditions.  $L$  varies with any linear dimension of the pipe. Now most whistles are made of brass. The coefficient of linear expansion of this metal is .000019.<sup>23</sup> But the coefficient of change of the velocity of sound in air with temperature is, as we have seen, .0018. Thus the change in length is only about one one-hundredth of the change in velocity, and may be considered negligible in comparison with the latter. For other metals, the coefficient of expansion is of the same order.

Since  $C$  and  $L$  do not change,  $N$  must vary directly with  $v$ , and must have the same coefficient of change with temperature, namely .0018. This means that the frequency changes .0018 or .18% for each degree of change in temperature, and gives us the following formula for  $N_a$ , the frequency at temperature  $t_a$ , when we know  $N_b$ , the frequency at the temperature  $t_b$ :

$$N_a = N_b [1 + .0018 (t_a - t_b)].$$

By means of this formula we can make any correction for temperature.

Ellis<sup>24</sup> gives a similar formula, for Fahrenheit temperature, in which the coefficient is .00104. If we change this to centigrade by the equation:  $1^\circ C = 9/5^\circ F$ , it becomes .00187, which is practically our coefficient.

Such a variation in pitch is by no means negligible. This is readily seen from the following table, which gives the magnitude of the variation at various parts of the scale for  $1^\circ$  and for  $10^\circ$ . A room is liable to vary in temperature, from day to day, as much as this latter amount.

Pitch in <i>vs.</i>	Variation in <i>vs.</i>	
	$1^\circ$	$10^\circ$
100	.18	1.8
500	.9	9.
800	1.4	14.
1000	1.8	18.
5000	9.	90.
10000	18.	180.
30000	54.	540.
50000	90.	900.

Let us now turn to the calibration of whistles and variators. If we are using a graphic method, such as the Marbe smoke-rings, we get a record of actual frequency at the temperature at which the determination is made. But if we are to compare a series of determinations (at different parts of the scale, for example), we must correct for temperature by reducing the frequencies obtained to the

<sup>23</sup> Watson, *op. cit.*, 299.

<sup>24</sup> *Loc. cit.*

frequencies at some standard temperature. This may be readily done by the type of formula given above, namely:

$$N_s = N_r [1 + .0018 (t_s - t_r)],$$

where  $t_r$  = temperature at which determination is made.  
 $t_s$  = standard temperature.  
 $N_r$  = frequency determined at temperature  $t_r$ .  
 $N_s$  = frequency at standard temperature  $t_s$ .

We may next consider calibration by comparison with a source of known frequency (setting by beats). If we use some source which does not vary with temperature, like a siren, which is dependent only on speed of rotation, we get a direct determination for the temperature at which we are working; and this can be reduced to the frequency at a standard temperature in the manner described above for use with a graphic method. But most of our sources of tone vary with temperature. A tuning fork, which is a usual standard source, varies .000112 of its frequency for each degree of change in temperature.<sup>25</sup> This is about one-twentieth of the variation of a pipe, and may usually be considered negligible in comparison with the latter. If, however, it is desired to make this correction, the temperature for which the frequency of the fork is stated must be known.

But when we compare with another whistle, pipe, or variator, the correction for temperature of the standard is no longer negligible. For all we know about our standard is that it gives a certain frequency at a certain temperature. Now let  $t_q$  be the temperature at which our known source has a frequency  $N_q$ . Then at the temperature  $t_r$ , at which the comparison is made, it will have the frequency  $N_r$ ; which, since the instrument is a wind instrument, is given by:

$$N_r = N_q [1 + .0018 (t_r - t_q)].$$

We set the instrument to be calibrated so that it has this same frequency,  $N_r$ . We must then reduce this to  $N_s$ , the frequency at the standard temperature,  $t_s$ , for which we are going to calibrate our unknown whistle or variator. We proceed as follows:

$$\begin{aligned} N_s &= N_r [1 + .0018 (t_s - t_r)] \\ &= N_q [1 + .0018 (t_s - t_r)] [1 + .0018 (t_r - t_q)] \\ &= N_q [1 + .0018 (t_s - t_r) + .0018 (t_r - t_q) + (.0018)^2 (t_s - t_r) (t_r - t_q)]. \end{aligned}$$

But the factor  $(.0018)^2 (t_s - t_r) (t_r - t_q)$  is negligible, and is, in fact, neglected in practice. Therefore

$$\begin{aligned} N_s &= N_q [1 + .0018 (t_s - t_r) + .0018 (t_r - t_q)] \\ N_s &= N_q [1 + .0018 (t_s - t_q)]. \end{aligned}$$

This expression does not involve  $t_r$ , the temperature at which the comparison was made. This means that we can make our comparison regardless of the room temperature, and then calculate the frequency of our unknown source for any temperature we may care to assume ( $t_s$ ). In the special case, where we want to calibrate our unknown for the same temperature as that for which the known source is calibrated,  $t_s = t_q$ , and no correction for temperature is necessary. Of course, this all assumes that both sources of tone are blown at the same temperature.

<sup>25</sup> Barton, *op. cit.*, 299.

This independence of the temperature at which the work is done also holds for calibration by the method of Kundt's dust-figures. For the frequency calculated by this method is:<sup>26</sup>

$$N = \frac{v}{2l}$$

where  $v$  = velocity of sound in air at the existing temperature

$l$  = distance between nodes.

But this is the frequency given by the whistle; and if it is blown at the same temperature, this is:

$$N = \frac{Cv}{L}$$

therefore

$$\frac{Cv}{L} = \frac{v}{2l}$$

and

$$\frac{C}{L} = \frac{1}{2l}$$

since the velocities are equal, being those of air at the same temperature. This shows us that, in calibrating a wind instrument, the distance between the nodes depends only on the construction of the instrument, and is independent of the temperature, provided that both the dust-tube and the source of sound are at the same temperature. This is, indeed, to be expected; for the frequency of the whistle or variator varies directly as the velocity of sound in air, which is the factor that varies with temperature. And the frequency varies similarly when calculated from the dust-piles. Therefore they both vary together for any change in temperature, and the ratio of the other factors must needs remain constant. This is the same independence of the temperature at which the determination is made that we found in the case of comparison with a wind instrument of known frequency. For any given whistle, we get the same  $L$  at any temperature. We may therefore assume that it was obtained at the temperature for which we desire to calibrate our whistle,  $t_s$ , and may then calculate accordingly, by the formula:

$$N = \frac{v_s}{2l}$$

where

$v_s$  = velocity of sound in air at temperature  $t_s$ .

$v_s = v_0 + 61 t_s$ .

$= 33150 + 61 t_s$

$= 33150 [1 + .0018 (t_s - 0)]$ .

All of these methods give us calibrations of our whistle or variator at some standard temperature,  $t_s$ . If it is used at some other temperature ( $t_p$ ), it is merely necessary to correct for that temperature

<sup>26</sup> *Ibid.*, 534.



in order to get the actual frequency. This can be done by the formula:

$$N_p = N_s [1 + .0018 (t_p - t_s)].$$

To sum up, it has been shown: (1) that the frequency of a whistle or variator varies appreciably with temperature; (2) that temperature must be taken into account in calibrating one of these instruments; (3) that calibrations at different settings must be reduced to a standard temperature to be comparable; (4) that in calibrating by comparison with another wind instrument the temperature at which the comparison was made need not be considered, since both instruments vary to the same amount with temperature; but that the results of such a comparison are valid only for the temperature for which the standard was calibrated, unless a correction be made; and (5) that in calibrating by Kundt's method the temperature at which the determination is made is likewise of no account, the results being valid for the temperature for which the velocity of sound is taken in computing the frequency. Moreover, (6) formulae have been given for performing the various corrections.

## XXV. VISUAL QUALITY AS A DETERMINANT OF CLEARNESS

By J. S. SMITH

Are there certain color-qualities which make a special appeal to attention? If, for example, red and yellow were to appear simultaneously in the visual field, should we tend definitely to regard the one and to disregard the other; or, in general, is there any visual quality to which we are predisposed to attend? Gale<sup>1</sup> found that black on a white, and green on a black background are most effective for men, while for women red is the attracting color irrespective of background. But Gale apparently made no attempt to control the time of exposure, or to avoid successive contrast, adaptation, and the space-error. Furthermore, he employed only artificial illumination, and he failed to take introspections. It seemed advisable, therefore, to repeat the experiment.

*Method and Apparatus.*—We employed the method of paired comparisons. The stimuli were six colors of the Milton Bradley Spectral Scales: red (R), orange (O), yellow (Y), green (G), blue (B), violet (V), together with black (Bk) and white (W). These we mounted in one-inch squares, half an inch apart, on both black and white cards, five by eight inches; the black square was mounted on white, and the white square on black. The stimuli were presented in the Whipple tachistoscope, with an exposure of 160 sigma, in both daylight and artificial light, with dark adaptation. Care was taken that no color should appear in consecutive exposures. Since every color was presented with every other color on each background, there were 21 pairs of stimuli in each set. The cards were also shown inverted, to avoid any space error; so that for each illumination there were 42 observations with each background. Finally, every

<sup>1</sup> H. Gale, *Psychological Studies*, 1900, 55 ff.

series was shown three times over to every observer under all four conditions; that is, every observer made in all 504 judgments.

The observers, Mr. L. G. Meads (*M*), graduate student in psychology, Dr. W. S. Foster (*F*), instructor in psychology, and Miss M. F. Sturges (*S*), student in psychology, were instructed to report which of the colors shown was the more clear; if both were equally clear, they were to report 'equal.' After every series the observer was asked to report his experience in making the judgment of clearness.

*Results.*—Since every color was shown with every other color thirty-six times on each background and in each illumination, the frequency with which any one color may be judged as more clear varies between thirty-six and zero. The following table shows the frequencies of judgment and the rank-order of judgments, with both backgrounds and both illuminations, for each one of the three observers:

TABLE 1.

FREQUENCIES								RANK ORDER						
Black background (daylight)														
	R	O	Y	G	B	V	W	1	2	3	4	5	6	7
<i>F</i>	0	25.5	28.5	20	8	10	34	W	Y	O	G	V	B	R
<i>M</i>	0	24	30	19	12	6	35	W	Y	O	G	B	V	R
<i>S</i>	0	33	28	21	13	6	25	O	Y	W	G	B	V	R
White background (daylight)														
	R	O	Y	G	B	V	Bk	1	2	3	4	5	6	7
<i>F</i>	15.5	23	6	29.5	25	14	13	G	B	O	R	V	Bk	W
<i>M</i>	29	20	0	8.5	23	11.5	34	Bk	R	B	O	V	G	Y
<i>S</i>	9	36	11	27	26	13	4	O	G	B	V	Y	R	Bk
Black background (artificial light)														
	R	O	Y	G	V	B	W	1	2	3	4	5	6	7
<i>F</i>	19.5	31	24.5	14	7	5	25	O	W	Y	R	G	B	V
<i>M</i>	14	28.5	25	17	0	6	35.5	W	O	Y	G	R	V	B
<i>S</i>	26	35	24	13.5	8.5	4	15	O	R	Y	W	G	B	V
White background (artificial light )														
	R	O	Y	G	B	V	Bk	1	2	3	4	5	6	7
<i>F</i>	27.5	15.5	0	17	24.5	13	28.5	Bk	R	B	G	O	V	Y
<i>M</i>	32	18	0	15	18	14	29	R	Bk	B	O	G	V	Y
<i>S</i>	36	25	0	15	23	9	18	R	O	B	Bk	G	V	Y

The following excerpts from the introspective reports afford some indication not only of the nature of the experience but also of the criteria of judgment.

F. "When the exposure comes, one of three things happens: (1) I may really see only one color. This is particularly so where Y and O appear with a color considerably darker (on a white background); the latter is then seen first. (2) I may see both colors simultaneously, and clearness is equal. (3) I may see both colors simultaneously and one is clearer than the other. This is the most common occurrence." (Black background.) "Almost without exception the brighter color has been the clearer." "It seems to be the more intense color that is more clear, *i. e.*, the color which is brighter and more saturated. In general the colors are absolutely more clear." "I think that color on black would be more clear than color on white." "In general the more clear color is also the more distinct color." (White background.) "I have a general impression that attention goes to the more saturated colors and to the darker colors and, judging mostly from black and red, that saturation has greater compelling power than darkness."

M. (Black background.) "I seem to take the brighter color,—the one which was glary. I noticed no tendency to choose any particular color. I thought of a spot just behind the fixation point on the screen, and then I named the color which seemed to force itself upon me, the one which furnished the greater tendency to turn to, to attend to."

S. (Black background.) "Tendency for the light colors to be more clear; O looked shiny and was clearer than R. I remember that on the white background R was clearer." (White background.) "When there was light and dark color, the dark usually stood out; Y never got my attention (with artificial light), V only when darker than the other color. When R appeared with other colors it was seen at once and the others not noticed (the same thing was true when Bk and B appeared with light colors); R and O both seemed persistent, stood out immediately."

*Discussion of the results.*—The first impression made by the objective results is the large degree of individual difference. Two things are, however, obvious. (1) There was no predisposition for any one hue, which was common to all three observers. (2) The fact that observer S judged O as the more clear 129 times out of a possible 144 indicates an individual predisposition for that hue. Since no special predispositions can be clearly made out for the other two observers, and since the experiments with different backgrounds (all other conditions remaining the same) yield divergent results, there must be some factor, other than hue, which conditioned clearness. This conclusion is borne out by the introspective reports; the observers themselves believed that their attention was determined by lighter colors on dark backgrounds, and by darker colors on white backgrounds, and also by differences in chroma. We must therefore inquire as to the relative tint and chroma of the colors under the various conditions.

The arrangement of the stimuli in descending order of *tint* as measured by flicker photometry (the values in degrees of *black*) is as follows:

Daylight            Y (22), O (190), G (193), V (282), B (292), R (314).  
Artificial light: Y (21), O (114), G (234), V (288), R (301), B (317).  
To determine the relative *chroma*, we asked a number of psychologically trained observers to arrange the stimuli in chromatic order

under all the four conditions. There was general agreement for the colors on white, but disagreement for the black background. In both cases the colors fall into certain groups within which discrimination is difficult. With the white background, the descending scale (with the grouping) is for daylight, B, GO, YRV; for artificial light, GO, RB, YV. With black backgrounds the diversity was such that we give the actual judgments (with grouping):

Obs.	Daylight	Obs.	Artificial light
W.	B, O-G-Y, R, V	W.	G-R-O, B-Y-V.
B.	B, O-R-G-Y, V	R.	G-O-R, Y-B-V.
F.	O-B, G-Y, R, V	B.	O-G-R, Y-B-V.
C.	O-Y-R-G-B, V		

The safest conclusion is that, on a black background in daylight, the colors are of the same order of chroma, V alone excepted; while in artificial light they fall into two groups, within each of which the chroma is of the same order. The chroma of B and R seems to be especially sensitive to the objective conditions; while O and G maintain a high and V a low position under all four conditions. A similar result is found with regard to tint; Y and V are of approximately the same tint under both illuminations; G and O, which are nearly equal in daylight, differ by  $120^\circ$  of black in artificial light. These facts account easily enough for a general shift of clearness under the various conditions; but they do not explain the individual differences of judgment in any single series. Since we had no further control of the variable factors, and since the number of judgments is too small to render the differences upon which the rank-orders are based positively significant, our data are inadequate to a quantitative treatment. We can offer only a qualitative and tentative explanation.

We suppose that our observers were differently disposed toward the different factors, and that the dispositions were variously actualized by the objective conditions. For example, *in the experiments in daylight and with a black background, we find a general agreement among observers*; the rank-orders are the same for F and M, except for a one-place transposition of colors nearly equal in tint; while observer S differs from M only in the exchange of positions of O and W. Apart from these transpositions, *the reports of all observers reproduce the descending scale of tint*. We have seen that, under the conditions of the series, the colors (V excepted) are of the same order of chroma. The objective conditions would therefore favor predispositions for hue and for light-contrast, and would inhibit a predisposition for chroma. The predilection of S for O appears; otherwise, contrast-effect seems to be the sole condition of attention for all three observers. *On the white background, however, chroma apparently becomes more effective*; and there seems to be no reason why, with any given pair of colors, either hue, tint or chroma (or a combination of them) should not be determining, according to the predisposition of the observer at the moment. If this be true, we might expect disagreement among observers under the conditions of our own experiment.

*Summary.*—(1) We have found no predisposition for any one hue, common to all observers; we find evidence, however, of a predisposition for a certain range of hues in the case of one observer.

(2) The problem is not so simple as Gale thought it to be. Aside from hue, tint or chroma, or both, may also be effective to determine attention; and the importance of the variable factors depends partly upon objective conditions, and partly upon the predisposition of the observer. The judgments are not difficult; and now that we have some idea of the factors involved, a repetition of the experiment with stricter control of stimuli should yield a satisfactory answer to our general question.

## XXVI. A PRELIMINARY STUDY OF VOWEL QUALITIES

By J. D. MODELL and G. J. RICH

The experiments reported in this paper were undertaken as a test of Köhler's earlier work with only approximately pure tones. Köhler employed tuning forks as stimuli; and his observers reported that the series of tones ranging from 163 to 4,000 v. d. gave a series of vowel qualities which could be arranged like those of color. Just as in the case of color there is a series of qualities from red through several nuances of orange to yellow, and from yellow a second series to green, and so on, so there runs a series in the phenomenal tone-system from *u* (moon) through *u-o*, *o-u* to *o*, and from *o* through *o-a*, *a-o* to *a* (father), and so on through *e* (prey) to *i* (machine).<sup>1</sup> In later experiments, in which he used Stern variators with interference apparatus, Köhler determined more accurately the pitch of the pure vowels; added those of *s*, *f*, and *ch* to the upper, and of *m* to the lower region of the scale; and concluded that the pure vowels fall regularly in octaves.<sup>2</sup> Since, however, we wished to know whether observers untrained in continental vowels would give the same judgments, and since Köhler himself holds that vowel qualities are to be found in all relatively simple tones, we have restricted our investigation to the preliminary aspects of the problem.

*Apparatus.*—We used as stimuli four Stern variators which gave a total range of 100-1200 v. d., a piston-whistle of 1072-3400 v. d., and an Edelmann-Galton whistle from 3560 to 5770 v. d. The variators were tuned by beats with König forks, and the two whistles by the Kundt dust-method. In our first experiments the whistles were blown by pressure on a rubber bulb; but since the pressure could not be rigorously controlled, we later actuated all instruments by air from a tank-gasometer. Weights were added, to give sufficient pressure for the high variator and the whistles. A record was kept of the amount of pressure both at the tank and at the mouth of the instrument, since it was found necessary to adjust the amount of pressure for the particular instrument if we were to obtain the best quality of tone. The pressure varied from 1.8cm. of water for the 300-600 variator to 14.0cm. for the Galton and 15.0cm. for the piston-whistle. All instruments were so connected with the tank that they could be instantly actuated at full pressure. There were two sources of error in the apparatus; a constant error of not more than 1%

<sup>1</sup> W. Köhler, *Akustische Untersuchungen*, II, *Zeit. f. Psych.*, lvi, 1911, 59 ff.

<sup>2</sup> See also *Zeit. f. Psych.*, lxiv, 1913, 92 ff.

when the temperature was 22-23 C., due to imperfections in the variators; and a temperature error, which never exceeded 1.2%, due to changes in the temperature of the room in which the experiments were conducted. In spite of our attempts to perfect the apparatus, the series of tones was not uniform in quality; there was a decided difference in quality between the lowest variator and the next above, between the highest variator and the piston-whistle, and again between the piston and the Galton. Furthermore, the lower and higher tones of the 300-600 variator were weaker than the tones in the middle of its scale. We could not be certain, therefore, that the observer did not associate certain judgments with certain points in the scale.

*Method.*—A series of 48 stimuli was prepared, in which the tones advanced regularly by intervals of thirds and fourths from 100 to 268, by whole tones from 268 to 536, by semitones from 536 to 3570, and by whole tones from 3570 to 5770 v. d. Ten series, five ascending and five descending, were given alternately to every observer. To avoid habituation, one tone out of its regular order, but within the same range of pitch, was inserted after every four or five stimuli. The judgments on these extra tones became part of the record, but were omitted from the final results. The observers were Dr. E. G. Boring (B), instructor in the department; Mr. F. S. Kleinman (K), student in psychology; Mr. W. F. Edgerton (E), a student untrained in psychological observation; Mr. G. J. Rich (R), graduate student in psychology, and Dr. H. P. Weld (W), assistant professor of psychology. W alone had had training in music and phonetics. Two sets of instructions were employed. The first three observers above mentioned sat in an adjoining room which communicated with the sound-room by a speaking-tube, and were instructed to write down the vowel-sound which they heard. This method was discarded, because from lack of training in phonetics the observers wrote the vowels in many different characters; so that in the end we were forced to have the reports interpreted, in conformity with continental pronunciation, before the judgments could be understood. This difficulty was obviated by the second method. The observer now sat in the room with the apparatus, though in such a position that he could not see it, and was instructed to reproduce the vowel which he heard or (where strict reproduction was vocally impossible) to pronounce the judgment. All observers were given extended practice before the experimental work began. They were told to regard the tone as singing or speaking a vowel to them. We began with *u* and *i*, as Köhler suggests; then added the remaining vowels; and later took many short series of tones at various parts of the scale. We found it necessary also to point out to the observers the American peculiarities of the vowels *a* and *e*, the former being rarely pure, and the latter having a decidedly diphthongal character. The procedure in the experiments proper was as follows. The experimenter gave the ready-signal, and in about two seconds produced the first tone of the series; the duration of the tone was roughly one second; it was repeated two or three times, or oftener if the observer desired; the judgment was recorded, and the apparatus was set for the next observation. The single experiment did not exceed two minutes, and in the later stages of the work a complete series was run off in thirty-five or forty minutes.

*Results.*—We found that, under the instructions of the experiment, all observers were able to hear vowel qualities in tones; and, with

one exception, all gave results similar to those of Köhler. Aside from *a*, observer E reported no pure vowels; the mixed forms, *u-o*, *o-a*, *a-o*, etc., were reported, however; and it seems probable that habituation carried E beyond the pure vowel to a point at which the new quality appeared. We have attempted to find the pure-vowel values by averaging the pitch-numbers for which he gave a judgment either of the vowel itself (*a*), or of a combination in which the vowel predominated, every such number being weighted according to the number of judgments. For the other observers, the results are the frequencies of the different judgments for every tone of the series. For our present purpose we need consider only the cases in which the judgment was that of a pure vowel. To obtain an average value for every vowel, the frequencies of the tones judged as a given vowel have been averaged, and each one weighted by the number of relevant judgments. The mean variation has then been calculated for each one of these averages, the single values being weighted in the same way. We have also averaged the values of every vowel for all observers, and finally have averaged the M. V. For the sake of comparison, we have worked out by the same method Köhler's results for his first series with himself as observer. All these results are shown in the following table:

Observer	VOWEL				
	<i>u</i>	<i>o</i>	<i>a</i>	<i>e</i>	<i>i</i>
B	229 ± 49	576 ± 46	1057 ± 209	1778 ± 191	3455 ± 293
K	340 ± 48	630 ± 72	1378 ± 194	1875 ± 75	3941 ± 372
E	(273)	(904)	(2425)	(3569)	(4360)
W	267 ± 36	588 ± 62	1233 ± 177	1967 ± 150	4141 ± 355
R	287 ± 36	606 ± 48	1103 ± 122	1997 ± 110	3439 ± 460
Köhler	223 ± 56	468 ± 85	1139 ± 95	2265 ± 233	3480 ± 409
Average of B, K, W, & R	281 ± 33	600 ± 18	1193 ± 113	1904 ± 79	3744 ± 297
Average of M. V. for B, K, W, & R	42	57	175	131	370

No M. V. was computed for observer E, because it would be in no way comparable with those of the other observers. The averages themselves are only very roughly comparable.

If we look at the individual results, we find that the four observers stand in almost the same order as regards the relative position of the different vowels in the scale. K is usually the highest; then come R, W and B. There are only three inversions of this order, one for *a*, and two for *i*. *Some observers, then, tend to place the whole series of pure vowels higher than others.* When we compare the average of the four with Köhler's results, we find that it is somewhat higher for *u*, *o*, *a*, and *i*, and lower for *e*. The M. V. also shows large differences. Considered relatively to one another, the separate values indicate the size of the region over which the pure vowel is heard; but this is, of course, much smaller than the actual size of that region, because the values near the average have the greatest weight.

If we consider the averages of the four observers, *an octave-relation can be made out between u, o, and a, and between e and i, but hardly between a and e.* It is surprising that the M. V. of *e* is smaller than that of *a*; we had expected that *e*, owing to its naturally diphthongal character, would be the more troublesome.

Results of this sort can do nothing toward settling the current controversy regarding the nature of the vowel-sounds. We thought however that, if Köhler's theory is correct, then the fact of vocality ought to appear fairly plainly in tones such as those which we employed, and ought to appear for observers of various type and training. It does appear; and even if the turning-points are in some measure determined by the inequalities of our tonal series, still they are reported as turning-points of vowel-change; the pitch-numbers may have been somewhat shifted, but the vocality remains. Whether Köhler is right or wrong, it is noteworthy that vocality can at all, with this degree of consistency, be heard into or heard out of the tones employed.



## A BIBLIOGRAPHY OF RHYTHM<sup>1</sup>

By CHRISTIAN A. RUCKMICH

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## BOOK REVIEWS

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*Native Tribes of the Northern Territory of Australia.* By BALDWIN SPENCER. With illustrations. London, Macmillan & Co., 1914. pp. xx, 516.

A classic does not repeat itself; and we shall not thrill again as we thrilled to the story of the Arunta. They, alas! are "decimated in numbers and hopelessly degenerate in customs;" indeed, "there are very few parts of Australia now left in which it is possible to study the aboriginal in his natural state." All the more do we owe gratitude to Professor Spencer for this third contribution to Australian anthropology.

The present work is mainly concerned with the natives of Bathurst and Melville Islands, and with the tribes of the Kakadu nation, inhabiting the Coburg peninsula and the district drained by the Alligator Rivers, some hundred odd miles east of Port Darwin. From the Islands comes an account of burial and mourning ceremonies, the most elaborate on record. They are "interesting because they differ so completely from any on the northern mainland and seem to point to the fact that the island natives have either developed these ceremonies among themselves or have derived them from some other people with whom they, but, apparently, not the natives of the mainland, have come in contact." In most respects the islanders are closely allied to the mainland tribes, so that "the existence of these remarkable burial and mourning ceremonies on the two islands is very difficult to understand." Space may be spared for a single detail: the women carry, besides discs and armlets, highly decorated rings (flat discs with the central portion cut out), some of them as much as 15 in. in diameter,—curiously suggestive, therefore, of the wreaths of civilization; and "the whole color-scheme and design of these armlets, discs and rings [which are shown in color] is quite distinct from anything met with on the continent." The Kakadu and allied tribes add to our knowledge of initiation ceremonies, especially by way of a final ceremony known as Muraian, with which is associated the use of a curious array of sticks and stones connected with totemic animals and plants.

These instruments are instructive, as representing a possible stage on the way from copy to convention: "it is well known how copies, taken in succession, one after the other, of an original drawing, become so modified in the course of a relatively small series, that the last bears no resemblance to the original; . . . it is possible that the Muraian objects show us a series that have not as yet become completely conventionalized but are on their way to this."

We recognize the heel of Achilles in the tale of a man "who had not been completely red-ochred. There was just one spot on his foot which had been left untouched, and here . . . a snake had bitten him, . . . in proof whereof he showed me the scar." Our attention is caught, again, by the plate of pot-hole-like depressions in a rock surface, that are "clearly of human manufacture," and yet unlike anything found anywhere else in Australia; "they seem

to be the work of a people inhabiting the country before the present natives came." Finally, mention may be made of the plates of rock and bark drawings (men, animals, and mythical beings or Mormo) included in the chapter on Decorative Art.

Professor Spencer laments that he has lost the cooperation of Mr. Gillen, to whose memory he dedicates, the book. Mr. Gillen's death was, it is true, a serious blow to anthropological science; but the surviving author has well maintained the standard of the two previous works. The chapters deal, in what is now familiar fashion, with social organization, marriage and initiation; totem groups and ceremonial objects; burial and mourning ceremonies, magic and medicine; customs, beliefs, traditions; food restrictions; weapons, clothing, implements and art. An appendix contains vocabularies and language notes. There are 92 photographic reproductions, beside text-diagrams and maps; and there are 36 plates, of which eight are in color.

E. B. T.

*The Khasis.* By P. R. T. GURDON. With an introduction by Sir C. LYALL. Second ed., illustrated. London, Macmillan & Co., 1914. pp. xxiv, 232.

The Khasis inhabit the Khasi and Jaintia Hills, an Assamese district of some 6,000 square miles whose capital is Shillong. They are isolated among an encircling population of Tibeto-Burman stock; and who they precisely are, and where they came from, are still unsettled questions. Their nearest affinity is to the Mon people of Further India, and they may have moved into Assam from the south-east, linguistic evidence is accumulating, but general anthropological data are still to seek.

The Khasis present three distinctive features which recommend them to the anthropologist. "In the first place, their social organization presents one of the most perfect examples still surviving of matriarchal institutions." Secondly, they possess remarkable methods "of divination for ascertaining the causes of misfortune and the remedies to be supplied. . . . It is somewhat surprising to find among them the identical method of *extispicium* which was in use among the Romans, as well as an analogous development in the shape of egg-breaking . . . which seems to have been known to diviners in ancient Hellas." Thirdly, they have "the custom, which prevails to this day, of setting up great memorials of rough stone, of the same style and character as the *menhirs* and *cromlechs* which are found in Western Europe, Northern Africa, and Western Asia." All these things are set forth, with ample illustration and in full detail, by Colonel Gurdon, under the headings of General Description, Domestic Life, Laws and Customs, Religion, Folk-lore (typical folktales, in translation and in the original, are given). Miscellaneous (naming of parents from children; method of calculating time; characters and customs of the Lynggams), and Language. Three appendices deal with the exogamous clans of the Cherra and Khyrim states, and with divination by egg-breaking.

The work was first published, as a government monograph, in 1907. It has now been brought up to date, especially upon the side of language, furnished with a bibliography, and illustrated by ten colored plates from water-color drawings. The book further contains a map, and a number of photographic reproductions. It is a member of the series which contains Colonel Shakespear's monograph on the Lushei Kuki Clans (this JOURNAL, xxiv., 287).

*Orchestration.* By C. FORSYTH. London, Macmillan & Co.; New York, The Macmillan Co., 1914. Pp. xi, 517.

This work, a volume in The Musician's Library, is written as a guide to students of music who are composing for the orchestra. The author's plan is "first, to describe our modern orchestral instruments, where they sprang from, how they developed, and what they are to-day; next, to trace the types of music which have been reflected in these constructional changes and, in especial, the types most familiar since Beethoven's time. Without some knowledge on these points the student is working in the dark." "The main-lines of study concern the original type of instruments, then its modifications, and last its use in its present-day perfection,—or in some cases, one must say, very partial perfection. A good deal of space has been devoted to explaining the String-technique. This is a subject not often studied from the outsider's point of view." The book opens with a list of instruments and a table of compasses; next comes an introductory chapter on classification; and then the four principal divisions of the work take up, in order, the instruments of percussion, the brass, the wood-wind and the stringed instruments. The writing, though condensed, is clear, and leavened with a pleasant touch of humor; there are nearly 300 illustrative excerpts (more or less compressed) from modern scores. I am not competent to appraise the volume on its technical side; but I have found it useful and interesting from the side of the psychology of music; it is, indeed, precisely the type of technical work upon musical composition that the psychologist needs. The author never loses sight of historical continuity; and a remark like the following (there are many such remarks) is illuminative: "a seventeenth-century Horn-player, if we could resuscitate him, would probably be considerably astonished at the Horn-playing which he would hear at a present-day Symphony-concert. But if he were a good Horn-player, it would not be many weeks before he would be quite competent to 'make one' in the orchestral quartet." There are also many valuable observations upon the feelings, simple and subtle, aroused by instrumental color and by phrasing. I do not hesitate to recommend the book to experimental psychologists. A second edition might be improved by the addition of a systematic bibliography.

E. B. T.

*An Introduction to General Psychology.* By R. M. OGDEN. London, Bombay, Calcutta, and Madras, Longmans, Green and Co., 1914. Pp. XVIII, + 270.

As an apology for lengthening the list of textbooks in psychology, the author acknowledges two principal motives: (1) to furnish a general elementary text which will "supply the student with the sort of introduction into the science of mind that will enable him, on the one hand, to connect his psychology with everyday life, and, on the other hand, to apprehend the bearings of this science upon philosophy, education, sociology, and biology," and (2) to act upon the "conviction that the time has come when we must modify some of our psychological principles and conceptions, with reference to the more recent investigations of the thought-processes."

In regard to the first motive it must be admitted that the author is in good company. The last few years have shown unmistakable indications of a tendency to make the science of psychology less abstract and to reinstate the system of values which obtains in experiences of common life. The problem as to the wisdom of de-

ferring to the interests of students in the questions of the day, or of presenting scientific facts for their own sake in the uncouth rigor of the science, an author must solve for himself. Personally, the reviewer believes that a compromise may be made by studiously selecting as illustrations of the facts and laws of the science those incidental experiences of daily life which most easily lend themselves to such interpretation. Under this sort of treatment psychology would certainly be spared the fate of being required to masquerade in the ill-fitting garments of other disciplines.

With respect to the second motive, the reviewer cannot escape the feeling that the dogmatic presentation of thought-elements is still premature. It was but a few years ago that the same author specifically stated his inability to obtain adequate introspective descriptions of continued thought-processes in terms of content. We now find the unconscious *task* and the *determining tendency* either made part of the conscious element, thought, or left unexplained, and the thought-element is presented to us without attributive description or differentiation from other elements. May it not again be said that the explanation for this procedure is to be found in "the unconscious bias of laboratories" directed by adherents of the school to whose founder the present text is inscribed?

In Part I psychology is defined as a study of mental happenings which takes into account (1) structural contents of consciousness, (2) psychophysiological relations, and (3) purposive mental organization and function. Very wisely the final definition of consciousness is left in terms of the entire material of the book. Consciousness has unity, bears personal reference, and may, under certain conditions, be called a soul. The methods which are used to analyze and describe mental life are retrospection, which is the more proper form of introspection, and to some extent the method of behavior. The importance of the *problem*, of the *directive selection* of processes, and of *interpretation* of results in observational description is emphasized.

Part II describes the analytical facts of mind under the usual headings of the sense departments. Some of the queries which arise from the study of these chapters are: (1) Is it consistent to speak of a colorless series as having color tone? (2) Why confine adaptation to local stimulation in vision? (3) Is not 20,000 vd. too low for the average upper limit of tone? (4) Are timbre, mellowness, and shrillness correlative qualities? (5) Is not 'touch' an antiquated rubric for the cutaneous, kinaesthetic, and bodily sense-groups?

In Part III the synthetic facts of mind are considered. Attention depends in part on the nature of mental activity displayed in interest and in part on the nature of mental contents as described in terms of attributive clearness. Other facts of synthesis are memory, perception, ideation, reaction, and emotion. Except for occasional tendencies to introduce epistemological interpretations and to emphasize purposive direction of mental processes, these subjects are treated, for the most part, in a conventional manner.

The book concludes with Part IV, which summarizes the issues of psychology under the topics: 'mind and body,' 'personality,' and 'character.' In this section the author specifically leaves the empirical level of the science for the interpretative and theoretical, and even sallies into the speculative and evaluative realms of some of the philosophical disciplines.

Noteworthy qualities of the text are its clarity of style and its unequivocal diction. The book reads well. In many places, however,

one feels the need of a proportionate balance between chapters and of a careful elimination of contradictory statements, but future editions will doubtless not elicit this negative criticism. It is certain that the book will meet the requirements of a large number of small institutions where psychology is taught less from an experimental point of view than with an aim to provide the elementary student with materials for general culture.

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CHRISTIAN A. RUCKMICH.

*Psychology, General and Applied.* By HUGO MUENSTERBERG. New York and London, D. Appleton and Co., 1914. Pp. xiv + 487.

Another book which aims to present psychology less abstractly and with more regard for the problems of everyday life has appeared among the textbooks of an unusually productive year. After the introductory chapters, the volume is divided into sections on 'causal psychology,' 'purposive psychology,' and 'applied psychology.' A sharp demarcation is made between the first two, viz., between an objective, structural, and causal explanation of mental states and a subjective, meaningful, and purposive interpretation of the mental life of the self.

Under the caption of 'causal psychology,' the author comes to the conclusion that in the realm of conscious states "there is no direct causal connection possible and that it cannot be introduced by the construction of a subconscious mental machinery." The psychologist must recognize "the general postulate that every single mental state be understood as the accompaniment of a special brain process." After a discussion of the scope and methods of causal psychology, a brief account of the neural substrate is, therefore, given. The chapter on 'stimulation,' however, surprises us in that physiological stimulation does not receive so much attention as the heading would indicate; especially is this true of the lower and internal senses. 'Sensation' would probably be the more appropriate title.

The chapter on 'inhibition' gives occasion for discussing the well-known *action theory* in terms of which the facts of attention, apperception, emotion, thought, the *Aufgabe*, and the allied tendencies are here or elsewhere explained. The meaning of a perception is given by the preparation "for an adjusted line of action." This 'motor setting' is also responsible for the varying functions of the idea. But in space perceptions, the motor aspect, which Wundt emphasizes in his genetic doctrine, is held to be secondary to "the central process itself with which the motor reaction starts."

The closing chapters of this section describe personality, individual differences in the social group, and the union, submission, self-assertion, organization, and achievements of individuals. In the second section the purposive life of the individual and of social units is discussed under the separate headings of the soul, meaning, creation, and practical and ideal relations. In the last section the branches of practical, applied psychology are considered in the light of causal and of purposive psychology; but of the two, the reviewer notes the prominence of causal interpretation; in most cases the purposive aspects seem forced and unnatural and the illustrative citations seem to come from descriptive psychology.

The book is written in the author's customary readable style, but at the expense of concise and definite statement. The presentation suggests an enormous generalized fund, and an unusually comprehensive grasp of psychological data, but the absence of footnotes and



precise references often leaves the scientific reader's curiosity unsatisfied and the more mature student without the means to investigate specific problems. An extensive bibliography at the end of the book partially fulfills this need. More than any other book now before the psychological public, this volume shows the bearing of psychology on many phases of daily life and on the more closely allied disciplines. It points out in a very convincing manner that in the subject-matter of general psychology the consideration of the normative functions of the individual and of the social group has a place beside the description of the constitution of mind as a related series of psychic events. And it makes the further important point that the discussion of these two aspects must be kept rigorously separate.

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CHRISTIAN A. RUCKMICH.

*A First Book in Psychology.* By MARY WHITON CALKINS. Fourth revised edition. New York, The Macmillan Co., 1914. Pp. xxi + 428.

To pass through four editions in five years is a record equalled by very few other textbooks of psychology. It is a record indicative of an attempt on the part of the author to incorporate facts brought out in the development of the science, and, since the book stands practically alone in systematically presenting the doctrine of the *self*, the demand for new editions is further significant as a mark of the academic recognition of this interpretation.

In this edition Miss Calkins sets out to make her claim to a 'self psychology' more consistent in that the distinction between the science of mental processes as structural elements and the science of personal selves as related to objects has now been abandoned. The elements of consciousness, a term which has now become synonymous with 'personal attitude' are "constituents of all forms of the relatedness of self to its objects." Moreover, the book may be placed on the library-shelf labelled 'Behavioristic Psychology,' for, on reading some of the inserted passages we find that this relatedness can be interpreted in terms of behavior. "This somebody is not an isolated self but a self which is affected from without and which expresses itself in its behavior. In view of these facts psychology is more exactly defined as science of the self in relation to, or conscious of, its environment." But in the discussion of method, introspective analysis of experience under experimental control is still considered the mainstay of the science. Observation of behavior "belongs to ethics, to social science, and to pedagogy rather than to psychology," and is admitted only in so far as it premises self-introspection for purposes of inference.

The first five chapters deal with perceiving and imagining. These functions differ in that the first involves (1) passivity, (2) reflectively realized community of perceiving in other selves, and (3) relatedness to an object regarded as independently present. They agree in being (1) impersonal in reference to self, (2) particular in reference, and (3) chiefly sensational, rather than affective or relational, experiences. The imaginal types are given as visual, auditory, usually combined with motor-tactual, tactual-motor, and mixed; verbal types may also be classified into these groups. Perceiving and imagining may be analyzed by introspection into sensational elements, comprising eight large groups, visual, auditory, gustatory, olfactory, pressure, pain, temperature, and strain. Most of these groups contain elementary experiences of quality, intensity, and extensity. But these are

the products of reflective analysis; ordinarily fusions and assimilations are the immediate experiences. There are experiences which are still more complex, such as the consciousness of space, subdivided into the consciousness of apartness, of form, and of position; the consciousness of harmony, of rhythm, and of melody.

For the first time instinct, habit, and learning are considered in the main text. They are forms of bodily reaction under the laws of perceiving and imagining, and constitute ways in which the efficiency of the organism is promoted in terms of its environment. Other chapters on attention, memory, association, recognition, thought, emotion, will, faith and belief, and the religious consciousness, emphasize the relation of the self to its environment, as expressed by 'personal attitudes' which, under analysis, break up into structural elements.

With so much stress placed upon the relation of the self to its surroundings, we begin to wonder whether the author is not still a trifle inconsistent. From this latest presentation it appears that the relation of the self to its environment is best revealed through its behavior, and that introspection is best suited to the analysis of 'structural elements.' But the peculiar and ultimate method of the science of the relation of self to environment is not the method of behavior, but just this method of introspection. One may emphasize the method of description of behavior and then define psychology from this point of view; or one may lay stress upon the method of introspection and proceed to outline an introspective psychology; but, since the method employed to a large extent defines the science and delimits the results obtained, the reviewer does not believe it consistent to divide the accent as indicated. Thus again it happens, as Ruskin observed that "the thoughts of the wisest are little more than pertinent questions."

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CHRISTIAN A. RUCKMICH.

*The Rational Education of the Will.* By PAUL LÉVY. (Translated from the French, ninth edition, by Florence K. Bright.) Occult and Modern Book Center, Boston, 1914. pp. xvi + 241.

The author aims to show, in a manner comprehensive to any reader, how physical and mental diseases may be cured by the patient himself. The fundamental law of psychotherapy is that "every idea is action in a nascent state." After illustrating in detail the manner in which ideas tend to express themselves in action, the author takes up means of cultivating and strengthening auto-suggestion as the most efficient therapeutic method. At the outset the patient should become thoroughly familiar with his own mental and physical condition. He should accustom himself to think of auto-suggestion, and should practice it unceasingly, day by day, learning by his failures and making use of every encouraging emotional state. He should then develop a notion of what he is capable of becoming, and what he is capable of doing, constantly suggesting these things to himself. He should understand and apply the laws of mental and physical hygiene, and the law of habit formation. Training of the will consists in obtaining a systematic control of one's actions and thinking; for the will is the "result of numerous factors,—ideas, sensations and sentiments of all kinds." The will function according to the laws which govern the mind, hence the will is free to act only as it is submitted to these rules. Finally the author cites numerous observations which illustrate the therapeutic value of suggestion in the breaking of habits, in the curing of various

physical and mental disorders, and in the correcting of general moral and intellectual debility.

Although the book has but slight scientific value, and contains but little that is not generally known, it may be commended in that it presents a simple and systematic treatment of the subject, and in that it creditably serves the purpose for which it was written.

Clark University

RAYMOND H. WHEELER.

*Mentally Defective Children.* By ALFRED BINET and TH. SIMON. Translated by W. B. Drummond, with an appendix containing the Binet-Simon tests of intelligence by Margaret Drummond. New York, Longmans, Green and Co., 1914. 180 p.

Although the translation of this book, originally appearing in 1907, is somewhat tardy from the standpoint of those who for the last five years or more have been finding in Binet and Simon their inspiration for clinical and paidological work, nevertheless it is quite worth while from another aspect. It renders more easily accessible to schoolmen in general a book unique in its field. The subtitle, "Guide for the admission of abnormal children into special classes," is enlightening; the aim it implies is kept constantly in mind and only pertinent matters are discussed.

Beginning with a short chapter for orientation in the history of the work with feeble-mindedness, and the prevalence and significance of the condition, the authors present a three-fold classification of all abnormals as the mentally defective, the illy-balanced and the mixed groups. From the description given they seem to include under mentally defective the usual group so-called, the illy-balanced approximate the group now recognized as having only dementia tendencies, while the mixed group is made up of those aments having psychotic tendencies also. These groups are minutely characterized, their aptitudes are enumerated and the pedagogic conclusion is drawn that "all instruction given to defectives must be dominated by the question of its practical usefulness."

Since the need of a different kind of education for such children is obvious, educational segregation is the only solution. A systematic procedure for any such selection is necessary and the one here suggested is logical, not unnecessarily complicated and is based upon practical considerations. The first step is the selection of the group suspected of mental retardation. Since adaptability is a sign of potentially normal functioning the child who fails to adapt himself to the educational situation and therefore to pass along through the grades is probably abnormal. Of course a certain amount of retardation may be due to late entrance, illness or other similar causes. With such cases eliminated a list of all other children under nine years of age retarded as much as two years and of all over nine retarded as much as three years in the grades forms the group to be submitted to individual examination.

This examination is divided into three parts. First comes a pedagogical examination. Here are given Vaney's tests of reading, arithmetic and dictation ability with directions for their use, together with norms. These, with the exception of the arithmetic tests, would doubtless need evaluation because of language differences before they could be used on English-speaking children. The second examination is psychological. The then unfinished measuring-scale of intelligence of the authors is presented in its partial formulation and throws some

illumination upon the question as to how and why the scale was made. The medical examination is limited to an attempt on the part of the physician to discover the causes and possible alleviation of the condition already determined as abnormal by the two examinations mentioned above.

The fact is also emphasized that the value of the special school can not be determined accurately until extensive records are kept of the condition of pupils upon entrance and at regular intervals afterwards. The necessity of following the career of the pupil after he leaves the school is also emphasized.

The book is not purely scientific but scientifically practical and is an illustration of its closing statement that "methods of scientific precision must be introduced into all educational work."

FLORENCE MATEER.

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## BOOK NOTES

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*The feelings of man, their nature, function and interpretation.* By NATHAN A. HARVEY. Baltimore, Warwick & York, 1914. 276 p.

One takes up this book today with very great expectations, for the nature, function, and interpretation of feelings might be called, if not the chief theme of psychology in the present, the one sure to be so very soon. In sixteen chapters the author discusses the meaning of the terms and theories of feeling, expression, property, classification, aesthetics, relation to intellect, relation to consciousness, to memory, to attention, to will, to the ego, and finally discusses mental ontogeny. But one cannot resist the query what this author has been doing and how he has managed apparently to escape knowledge of the really epoch-making work done in this field in recent years. It is one thing to make a *chablone* for practical use in the schools, and another really to deal with a subject in a scientific way. Is it possible that we are developing, in this country, a normal school level of intelligence, ability, scholarship, or a normal school edition of knowledge? Let us hope not. I cannot conceive that there is anything in this book for the psychologist.

*The fundamentals of psychology; a brief account of the nature and development of mental processes.* By BENJAMIN DUMVILLE. Baltimore, Warwick & York, n. d. 382 p.

The writer first discusses the need of psychology as a basis of education, then the relations of mind to body, analysis of phenomena of sensation, perception, imagination, ideation (the latter in three chapters). Then come memory, conation, feeling, instincts and innate tendencies, nature and development of the sentiments, the will, and attention. The book cannot be said to be well gotten up in type, paper, or binding. Why should teachers not have the best there is in the lines they take up?

*The psychological methods of testing intelligence.* By WILLIAM STERN. Translated from the German by Guy Montrose Whipple. Baltimore, Warwick & York, 1914. 160 p.

It was a very happy idea of Professor Stern to condense his important work in this field into a brief handbook, and also of Professor Whipple to translate it. Here will be found just what a great many psychologists and teachers desire to know. After a brief introduction, discussing the problem of intelligence testing, the author passes to the single tests, taking up in detail the Binet-Simon method, its resultant values, its application to normal children, etc. Here the author discusses the general distribution of the levels of intelligence, age, nationality, social strata, school performance, sex differences, repeated tests with the same children. He then discusses abnormal children and the point of view of reorganization and improvement of gradation methods; the examination and testing of other gradations of intelligence come last.

*Children's perceptions; an experimental study of observation and report in school children.* By W. H. WINCH. Baltimore, Warwick & York, 1914. 245 p.

This is a book that is both timely and valuable. We have here various series of experiments, in school A on the work of children three, four, five, six and seven years old, and practically the same in school B. Then the work of different standards of boys and girls is considered, with a general chapter on the evolution of children's perceptual judgments. The writer then discusses how far the relative inferiority of the older children is due to differences in the methods of reporting, and gives a summary, with conclusions, and a statistical appendix.

*What ought I to do? An inquiry into the nature and kinds of virtue and into the sanctions, aims, and values of the moral life.* By GEORGE TRUMBULL LADD. New York, Longmans, Green & Co., 1915. 311 p.

*What should I believe? An inquiry into the nature, grounds and value of the faiths of science, society, morals and religion.* By GEORGE TRUMBULL LADD. New York, Longmans, Green & Co., 1915. 275 p.

In the first of the above volumes the author discusses the meaning of this question, whence comes the message "I ought," the intention of being good, doing one's duty, feeling I can, the worth of the moral ideals, many or one virtue, custom, law and the moral law, settling questions of conscience, the final issue, morality and religion.

In the second volume the author discusses guessing, "trowing," and believing, the will to believe, less and great beliefs, right and obligation to believe, comforts and rewards, right of belief, beliefs scientific and social, faiths of morality and of religion.

*Froebel as a pioneer in modern psychology.* By E. R. MURRAY. Baltimore, Warwick & York, 1914. 230 p.

This book was written with a notion that Froebel has not yet come to his own but will surely do so because his thought is so profound. He is said to have anticipated modern psychology, in his analysis of mind, his conceptions of early manifestations of will and consciousness, in the place he assigns to action in the development of apperception and feeling, instincts, play and work. The author gives an evaluation of Froebel's weak points and an answer to criticism, and in an appendix the meaning of activity is discussed. Perhaps he makes out as good a case as can be made for this mystic, whose intuitions and feelings were so deep but who really expresses so little. The author refuses to admit that almost everything in Froebel has been utterly superseded by modern child study, while it has steadfastly arraigned itself in opposition to this work.

*Die Grundlagen der Psychologie.* Von THEODOR ZIEHEN. Leipzig, B. G. Teubner, 1915. 2 v.

Ziehen begins with the epistemological basis of psychology, then discusses the *Gignomene* and its relations to the general principle of immanence. He seeks to comprehend psychic phenomena under the idea of the soul, to define its relations to the brain, the conscious and unconscious, and the relation of psychology to other sciences.

In the second volume the author follows what he calls the autochthone method, and describes this kind of psychology, working out its relations to apperception, *Vorstellung*, generalization, comparison, combination, inference, judgment, association, feeling-tone, will processes, etc.

*Educational guidance; an experimental study in the analysis and prediction of ability of high school pupils.* By TRUMAN LEE KELLEY. New York, Teachers College, 1914. 116 p.

This is a valuable contribution to a question of growing practical interest. How far is success in the grades evidence of high school ability, what is the relation of teachers' estimates to tests in the different departments or topics, and the culmination of tests with reference to the different topics? This book also discusses the factor of pupils' age, the comparison with other studies, practical application in high school classification, and guidance methods.

*Variation in the achievements of pupils; a study of the achievements of pupils in the fifth and seventh grades, and in classes of different sizes.* By CHARLES HERBERT ELLIOTT. New York, Teachers College, 1914. 114 p.

Here we begin with data based on tests of scorings in spelling and writing, composition, vocabulary, arithmetic, and other data. Then standards of achievement for the fifth and seventh grades, attainments in class, measurement of class size, are discussed, with an appendix directing how to administer these tests, samples of others, preliminary list of composition subjects, notes, and a bibliography.

*American thought from puritanism to pragmatism.* By WOODBRIDGE RILEY. New York, Henry Holt & Co., 1915. 373 p.

This volume treats of puritanism, early idealism, deism, materialism, realism, transcendentalism, evolutionism, modern idealism, and pragmatism. It is a useful introduction and will give the novice a helpful orientation, though it is extremely incomplete, with surprising gaps, and, as such a theme must inevitably do, displays the lines of both the author's reading and his ignorance, his likes and dislikes.

*Buddhist psychology; an inquiry into the analysis and theory of mind in Pali literature.* By MRS. C. A. F. RHYS DAVIDS. London, G. Bell & Sons, Ltd., 1914. 212 p.

This book contains nine chapters. The first is entitled Habits of Thought, while the following are devoted to the Psychology of the Nikayas. 1. On Mind in Term and Concept; 2. Consciousness in the External World; 3. Feeling; 4. and 5. Ideation. Then follows a chapter on Psychological Developments in the Pitaka. Chapter 8. is on Psychology in the Milinda. Then follow some mediaeval developments.

*Einführung in die experimentelle Psychologie.* Von N. BRAUNSHAUSEN. Leipzig, B. G. Teubner, 1915. 111 p.

This tiny primer treats of the history, justifications, and aids of experimental psychology, the psychic structures, sensation, intensity, Weber's law, apperception, judgment, *Vorstellung*, association, memory, *Aussage*, fantasy, attention, tests of intelligence, fatigue, aesthetics, will.

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Stephan Witasek was professor at the University of Graz. He published various papers on spatial perception, on learning and on other topics (one in collaboration with Meinong). He was the author of: *Grundzüge der allgemeinen Aesthetik*, 1904; *Grundlinien der Psychologie*, 1908; *Psychologie der Raumwahrnehmung des Auges*, 1910. In collaboration with A. Höfler, he published: *Psychologische oder experimentelle Psychologie an Gymnasien*, 1898; *Psychologische Schulversuche*, 1900; *Hundert Psychologie Schulversuche*, 1903.

Ernst Meumann was born August 29th, 1862. He studied under Wundt at Leipzig, where he received the Ph. D. degree in 1889. After serving as Privat Dozent for a time in Leipzig, he was called to Zürich, where he was promoted to the rank of professor in 1900. He was subsequently called to a chair in Königsberg, 1905; Münster, 1907; Halle, 1909; Leipzig, 1910; and to the Kolonialinstitut in Hamburg, 1911. Besides a number of papers on psychological and pedagogical topics, he produced the following works: *Die Entstehung der ersten Wortbedeutungen beim Kinde*, 1902; *Oekonomie und Technik des Gedächtnisses*, 1903; *Hausarbeit und Schularbeit*, 1904; *Vorlesungen zur Einführung in die experimentelle Pädagogik*, 1907; *Intelligenz und Wille*, 1908; *Einführung in die Aesthetik der Gegenwart*, 1911. He was founder and editor-in-chief of the *Archiv für die gesamte Psychologie* (since 1903) and of the *Zeitschrift für experimentelle Pädagogik* (since 1907).

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## THE HISTORY AND DERIVATION OF THE WORD 'FUNCTION' AS A SYSTEMATIC TERM IN PSYCHOLOGY<sup>1</sup>

By K. M. DALLENBACH

The New English Dictionary gives six current meanings of the word Function. Five of these may be traced back nearly four hundred years; the mathematical usage is of more recent date. In its etymological sense, and in the senses of mere activity, of a specialized activity, of a particular office, and of a ceremony, the word may be found in the literature of the 16th century.

We are here interested only in one of these meanings, that of a special kind of activity. Function in this sense is employed in three different ways; for the activity of an organ (this we shall call, for the sake of brevity, the physiological use); for the activity of the intellectual and moral powers or faculties (this we shall call the psychological use); and for the activity of things in general (this we shall call the general use). It is, again, only the broad psychological use that concerns us; the more narrow and particular meanings—in contemporary psychology the word is employed in several distinguishable senses<sup>2</sup>—will be thrown together under this single head.

It is our purpose to discover when the word first appears in English with a psychological meaning, and, if possible, whence it is derived. The New English Dictionary gives four

<sup>1</sup> From the Psychological Seminary of Cornell University.

<sup>2</sup> C. A. Ruckmich, this JOURNAL, XXIV, 1913, 99-123.

references. The first is to Shakespeare (1604): "As her appetite shall play the God with his weake function."<sup>3</sup> The second is to Milton (1671): "Nature within me seems in all her functions weary of herself." The third is to Coleridge (1809-10): "The functions of comparison, judgment, and interpretation." The last is to Farrar (1868): "The first function of the conscience is to warn." In the quotations from Shakespeare and Milton, however, the import seems ambiguous, and we cannot be sure that the passages anticipate our present-day usage.

We began our search with an examination of the works of the greater English associationists. Thomas Hobbes does not use the word in his "Elements of Philosophy" (1655).<sup>4</sup> An examination of the complete works,<sup>5</sup> letters, essays, tracts, and treatises of John Locke (1632-1704) shows that power and faculty, used synonymously, are favorite words with him; but he, like Hobbes, does not use the word function even in contexts where we might expect such use if the word were current in the philosophical discourse of the time. Locke, moreover, had had training in medicine,<sup>6</sup> so that in all probability the word was as rarely used in contemporary physiology. We failed also to find it in the works of Berkeley<sup>7</sup> (1685-1753), either in a psychological sense, or even in the sense of a public or religious ceremony. Hume's "Treatise on Human Nature" (1739) and "Inquiries Concerning the Human Understanding" (1777) were examined; but he, too, seems not to have employed the word upon a single occasion. Hartley, in his "Observations on Man" (1748), uses the term twenty times. In no case, however, does he employ 'function' in the psychological sense, unless the once-used expression "body and mind depend on each other for their functions"<sup>8</sup> be so construed. It is very unlikely that Hartley intends this implication; for, in other cases, the word is used either in a common-sense or in a physiological connection. He speaks, for example, of the structure and functions of the several organs, or of the parts; of the ordinary functions and actions of life;

<sup>3</sup> *Othello*, Act ii, Scene 3, line 344. The reference given by Bradley is to Scene 2.

<sup>4</sup> *The English Works of Thomas Hobbes*. Translation from the Latin by W. Molesworth, 1839, vol. i.

<sup>5</sup> *Works of John Locke*, 4th ed., 3 vols., 1740.

<sup>6</sup> A. C. Fraser, *Locke*, 1890, 19.

<sup>7</sup> A. C. Fraser, *Works of George Berkeley*, 4 vols., 1871.

<sup>8</sup> D. Hartley, *Observations on Man*, i, 30.

and of the clergy as not "discharging their functions" and "not pretending to any function or authority."

With Brown (1778-1820) the word comes into more general use. In his "Physiology of the Mind" it is found three times. Twice the term has a physiological,<sup>9</sup> but once, certainly, a definitely psychological application. "There is a PHYSIOLOGY OF THE MIND, then, as there is a *physiology of the body*; a science which examines the phenomena of our spiritual part simply as phenomena, and, from the order of their succession, or other circumstances of analogy, arranges them in classes under certain general names; as, in the physiology of our corporeal part, we consider the phenomena of a different kind which the body exhibits, and reduce all diversities of these under the names of a few general FUNCTIONS."<sup>11</sup> In the "Lectures on the Philosophy of the Human Mind"<sup>12</sup> the term is found in numerous passages, and is used variously in both senses. As applied to mental phenomena, it has come to be synonymous with the older terms, power and faculty. "The innumerable changes, corporeal and mental, we reduce, by generalizing, to a few classes; and we speak, in reference to the mind, of its faculties or functions of perception, memory, reason, as we speak, in reference to the body, of its functions of respiration, circulation, nutrition."<sup>13</sup> "The definition of the supposed power [power of conception] is sufficiently intelligible; but is there reason to add the power thus defined, to our other mental functions, as a distinct and peculiar faculty."<sup>14</sup>

This usage, as we have shown, could not have derived from the direct line of English psychologists. We have gone, therefore, to Brown's life and the references that he gives incidentally in the course of his lectures, to discover, if possible, its origin. From his biographies and his references<sup>15</sup> we find that he was influenced principally by Malebranche, Hume, Smith, Reid, Paley, Rush, Darwin, Condillac, de

<sup>9</sup> *Op. cit.*, vol. i, 7, 110, 42, vol. ii, 450, 452. The other instances of his use of the word may be found on pages 7, 18, 30, 42, 46, 52, 54, 110, 199, 266, 447, in vol. i, and 219, 450, and 452 in vol. ii.

<sup>10</sup> *Physiology of the Mind*, 1820, 51.

<sup>11</sup> *Op. cit.*, 2. Capitals and italics in original.

<sup>12</sup> 3 vols., edited posthumously by J. Stewart and E. Milroy, 1822.

<sup>13</sup> *Op. cit.*, vol. i, 14.

<sup>14</sup> *Op. cit.*, vol. ii, 104.

<sup>15</sup> D. Welsh, *Account of the Life and Writings of Thomas Brown*, M. D., 1825; D. Welsh, *Memoir of Dr. Brown*, in *Brown's Philosophy of the Human Mind*, vol. i, 1851; J. McCosh, *Scottish Philosophy*, 1874, 317-337.

Tracy, Gregory, and Dugald Stewart. Other names and references are to be found in his lectures, but the above are, unfortunately, the only sources at our disposal.

In the English translation of the "*Recherches de la vérité*" (1674) of Malebranche the word function appears only twice, and on both occasions in connection with physiological processes.<sup>16</sup> Hume has already been considered; he does not use the word in any sense. Adam Smith (1723-1790) does not use the word in his "*Theory of Moral Sentiments*,"<sup>17</sup> the only one of his works examined. The term is used three times in Reid's (1710-1796) "*Inquiry into the Human Mind*," but the applications again are in all cases physiological.<sup>18</sup> Benjamin Rush (1745-1813), the American physician, uses the word once only in his best known work, "*Diseases of the Mind*," where he speaks of function in connection with the will; suggestion from a physiological context rather than systematic psychological implication seems to be here responsible.<sup>19</sup> Rush gives no references, and we were unable to discover his intellectual antecedents. Brown read Erasmus Darwin's (1731-1802) "*Zoonomia*,"<sup>20</sup> and wrote a criticism of it.<sup>21</sup> Before publishing the criticism, however, at the advice of Dugald Stewart, he submitted it to Darwin, and a long correspondence sprang up. Darwin's influence on Brown could not have occasioned the use of the word 'function' in the psychological sense; for, in the four volumes of "*Zoonomia*," Darwin uses the word but twice, and then only in the physiological sense.<sup>22</sup> The word is not found in Paley's (1743-1805) "*Moral Philosophy*,"<sup>23</sup> nor in the three volumes of the works of Condillac (1715-1780) which were examined.<sup>24</sup> It is used once by Destutt de Tracy in the "*Éléments d'idéologie*," and then in the sense of office or duty.<sup>25</sup>

Stewart (1753-1828) was Brown's teacher and later his colleague and close personal friend; but it can hardly be

<sup>16</sup> N. Malebranche, trans. by T. Taylor, *Treatise Concerning the Search after Truth*, 1694, 33, 121.

<sup>17</sup> Published 1759; 10th ed., 2 vols., 1804.

<sup>18</sup> Published 1764; 3rd ed. 1769, 187, 219.

<sup>19</sup> *Medical Inquiries and Observations upon the Diseases of the Mind*, 1812; 3rd ed. 1827, 261.

<sup>20</sup> Published in 1794. 3rd ed., 1801.

<sup>21</sup> *Observation on Dr. Darwin's Zoonomia*, 1798.

<sup>22</sup> *Op. cit.*, vol. i, 16; vol. iv, 348.

<sup>23</sup> Published in 1785.

<sup>24</sup> *Oeuvres de Condillac*, 23 vols., 1798. The volumes examined were: vol. i, *Sur l'origine des connaissances humaines*; vol. ii, *Traité des systèmes*; vol. iii, *Traité des sensations*.

<sup>25</sup> Published in 1801. Translated, 1817, xx.

through him that Brown comes to speak of mental functions; for Stewart's single use of the term in three volumes of the "Elements of the Philosophy of the Human Mind" has reference to the body.<sup>26</sup> On the same page on which this quotation appears, however, Stewart quotes a long passage from the English translation (1801) of the introduction to Cuvier's "Leçons d'anatomie comparée"<sup>27</sup> and in this quotation 'function' occurs twice. But in the work of Cuvier published before Brown's death, 'function,' although used freely, is applied only to physiological processes, so that it could not have been Cuvier's influence which led Brown to his own use of the term. Our examination of Cuvier, however, gave us accidentally a hint as to the origin of Brown's usage. Cuvier, we found, was much impressed<sup>28</sup> by the doctrines of the new phrenology which were being disseminated at this time; and, as we shall attempt to show, it is *phrenology*, not by way of Cuvier, but much more directly, that was responsible for Brown's innovation.

Gall gave his first lectures upon phrenology at Vienna in 1796. They proved to be very popular, and he soon gained a following of students, foremost among whom was Spurzheim. Through the influence of the church Gall was forced to flee from Vienna in 1805 and, in company with Spurzheim, began a tour of Germany, lecturing and spreading his doctrines as he went. His expositions were popular and expressive, and he found many adherents. Spurzheim continued lecturing after his break with Gall in 1813, and traveled extensively through Germany, Switzerland, England, Scotland, and Ireland.<sup>29</sup> By his lectures and in other ways phrenological doctrines were widely disseminated; and though

<sup>26</sup> Dugald Stewart, *Elements of the Philosophy of the Human Mind*, vol. i, 1792; 2nd ed., 1802; vol. ii, 1813; 3rd ed., 1821; vol. iii, 1827. The passage referred to is in vol. ii, 3rd ed., 414.

<sup>27</sup> 10 vols. Edited posthumously by Dumeril and Duvernoy, 1835-45. (Only the first three volumes were examined.)

<sup>28</sup> In editing Cuvier's "Leçons," Dumeril refers by footnotes (vol. iii, 15, 70, 71, 72, 79, 117) to Cuvier's "Rapport à la classe des sciences physiques sur un mémoire de MM. Gall et Spurzheim, relatif à l'anatomie du cerveau" (1808). Cuvier's "Rapport historique sur les progrès des sciences naturelles depuis 1789 et sur leur état actuel" (1828) mentions Gall (p. 196) in connection with the advancement in physiology, and under the topic "The functions of the brain" (p. 232) calls particular attention to Gall. Henri Dehérain says in his "Catalogue des Manuscrits du Cuvier," that Cuvier was acquainted with Gall and Gall's doctrines as early as 1802.

<sup>29</sup> Articles on Gall and Spurzheim in *Encyclopedia Britannica*, 11th ed., 1910-11. See also Thomas Sewall, *An examination of Phrenology*, 1837, Lecture 1.

Brown makes no direct reference to phrenology in his writings, he was undoubtedly conversant with its doctrines. He was not only a physician and a trained physiologist himself, but his closest friend, James Gregory (1750-1821), was the most eminent physician of his time in Scotland.<sup>30</sup> Association with such a man would have kept him abreast of current physiological work and theory. We have, however, further evidence that phrenology was discussed in Edinburgh at the time. In 1815, Dr. Wm. E. Leach writes to Thomas Foster, who gave the name 'phrenology' to the new system and was then writing articles upon it: "Certain anatomists in London, and one in Edinburgh, have absolutely denied the truth of Dr. Spurzheim's observations on the structure of the brain and have pronounced them to be fanciful."<sup>31</sup> Furthermore, in 1815, an article "On the system of craniology of F. J. Gall and K. Spurzheim" appeared in the *Edinburgh Review*, a magazine to which Brown contributed. The article was denounced and laughed at for its seeming absurdities; but after Spurzheim's visit to Edinburgh, where he delivered a series of lectures on phrenology in refutation of his critics, the subject was more seriously considered. It gained many adherents and made many converts, foremost among whom were George and Andrew Combe. George Combe soon became active in the propagation of phrenological doctrine. His first article on the subject was published in 1817 in *Scot's Magazine*, and his first book in 1819.<sup>32</sup> In 1820 he helped to establish the Phrenological Society of Edinburgh.

In the phrenological works, as in Brown's "Philosophy of the Human Mind," functions of mind appear along with functions of body. In his first article<sup>33</sup> (1798), Gall speaks only of functions of the brain, the cortex, the nerves, and the different organs, and never of mental or intellectual functions. In his later writings, and in those written in collaboration with Spurzheim,<sup>34</sup> such expressions as "fonctions de l'âme," "fonctions intellectuelles," "fonctions de l'esprit," "fonctions de sens," are nearly as frequent as expressions of physiological

<sup>30</sup> David Welsh, *op. cit.*; *Encyclopedia Britannica*, locc. citt.

<sup>31</sup> *Pamphleteer*, v., 1815, 22off.

<sup>32</sup> G. Combe, *Essays in Phrenology*, 1819.

<sup>33</sup> F. G. Gall, Schreiben über seinen bereits geendigten Prodrum über die Verrichtungen des Gehirns der Menschen und der Thiere, *Wiand's Deutscher Merkur*, vol. iv., 1798, 311-335.

<sup>34</sup> Gall, *Introduction au cours de physiologie du cerveau*, 1808; *Recherches sur le système nerveux en général, et sur celui du cerveau en particulier*; F. J. Gall and J. C. Spurzheim, *Mémoire présenté à l'Institut de France le 14 Mars 1808*, 1809; *Anatomie et physiologie du système nerveux*, 1810, 1812.

significance. In the "Mémoire présenté a l'Institut de France" (1808), for example, we find the word 64 times, and 10 refer to mind. Spurzheim's independent writings show a similarly extended use. In his work on insanity,<sup>35</sup> which appeared in English in 1817, the word appears 152 times, and 49 have a direct psychological reference. These writers make function synonymous with the older terms power and faculty, writing at times "fonctions ou facultés."<sup>36</sup> Spurzheim characterized as 'particular' functions, combativeness, destructiveness, acquisitiveness, love of approbation, and the like.<sup>37</sup> Later writers upon phrenology continue the usage.<sup>38</sup>

It seems now sufficiently evident that Brown's use of the term function as applied to mind is a direct result of phrenological influences; for there can be no doubt either that such use was current in phrenology as nowhere else, or that circumstantial evidence shows Brown's interest in and familiarity with phrenology, in spite of his own lack of reference. We shall now attempt to prove that *there exists a remarkably constant relation between emphasis upon the importance of phrenological and physiological theory and the frequency with which English psychologists speak of mental functions.*

Sir William Hamilton (1788-1856) is careful to define the term. "With us," he says, "the word function . . . means, not the exercise, but the specific character of a power."<sup>39</sup> Hamilton uses the word frequently with reference to physiological structures, but, so far as we have found,<sup>40</sup> never with reference to mind, save in two instances where

<sup>35</sup> *Observations on the Deranged Manifestations of the Mind, or Insanity*, 1817.

<sup>36</sup> *Mémoire*, 229.

<sup>37</sup> *Lancet*, vii, viii, 1825.

<sup>38</sup> T. Sewall, *An Examination of Phrenology*, 1837, 57; H. Holland, *Medical Notes and Reflections*, 1839, 136, 141, 142, 146, 150, 151, 194, 196, 198, 204, 271, 274, 310, 312, 371, 373; S. G. Morton, *Am. J. of Science and Arts*, xxxviii, 1840, 356; O. S. Fowler, *Practical Phrenology*, 1850, 6, 7, 9; O. S. and L. N. Fowler, *Phrenology Proved, Illustrated, and Applied*, 1850, 8, 10ff.

<sup>39</sup> *Lectures on Metaphysics and Logic*, Edited by H. L. Mansel and J. Veitch, 1882, 180.

<sup>40</sup> We have examined John Veitch, *Memoirs of Sir Wm. Hamilton*, 1869; W. Hamilton, *Practical Consequences of the Theory of the Functions of the Brain of Dr. Gall*, 1826, 1827; Correspondence with George Combe, *Phrenological Journal*, iv, 1827, 377-407, v, 1828, 1-82; Physiological Researches in Relation to Phrenology, *Phrenological Journal*, v, 404-419; an article in Dr. Monro's *Anatomy of the Brain*, 1831, 4-8; *Medical Times*, xii, 1845, 177, 371 (a number of these articles may be found in vol. i. of Mansel and Veitch's edition of Hamilton's works); *Lectures on Metaphysics and Logic*.

he is discussing the systems of others. The one instance occurs in a discussion of phrenology;<sup>41</sup> the other in a discussion of Gassendi's philosophy.<sup>42</sup> Hamilton, to be sure, was a student of medicine, and retained his interest in physiology throughout his life; but far from accepting the tenets of phrenology, he was one of its most active opponents; so that the absence of the term 'mental function' in his writings confirms rather than opposes our thesis.

James Mill (1773-1836) seems serenely unconscious of the phrenological movement. In his "Analysis of the Human Mind"<sup>43</sup> he neither mentions Gall nor refers to phrenology. He does not consider the physiology of the nervous system, and with one exception makes no use of the word 'function.' On this occasion it is used in the sense defined by Hamilton, that of employment or office.<sup>44</sup>

In the writings of John Stuart Mill (1806-1873) the term appears infrequently, and usually with the sense of office, or with a physiological reference.<sup>45</sup> In three cases only does it refer to mind, and in all three it appears merely to be repudiated or denied in a discussion of another system (twice in that of Comte's new phrenology).<sup>46</sup>

August Comte (1798-1857), as we should expect from his well known phrenological leanings, often uses such expressions as moral and intellectual, affective, and phrenological functions.<sup>47</sup>

Appreciation of phrenological doctrine is also primarily responsible for the term function in the works of G. H. Lewes

<sup>41</sup> *Lectures on Metaphysics and Logic*, vol. i, 406, 411.

<sup>42</sup> *Op. cit.*, vol. ii, 202. Unfortunately, we were unable to obtain any of the original works of Pierre Gassendi (1592-1655), and are therefore unable to state definitely whether this use is Gassendi's, or whether it is Hamilton's. From the fact that the word does not appear in the "Three Discussions on Happiness, Virtue, and Liberty" (translated by Bernier, 1699), the only work of Gassendi's available, we incline to think that the use is Hamilton's.

<sup>43</sup> Edited by J. S. Mill with illustrative and critical notes by A. Bain, A. Findlater, and G. Grote, 1878.

<sup>44</sup> *Ib.*, vol. ii, 173.

<sup>45</sup> We examined *Sir William Hamilton's Philosophy*, 1865; *Auguste Comte and Positivism*, 1865; the editorial notes in the edition of James Mill's Works, 1878; the review of Bain's psychology, *Edinburgh Review*, cccxiv, 1859, 287-321. Examples of Mill's use may be found in *Sir William Hamilton's Philosophy*, 151, 365; *August Comte and Positivism*, footnote 82, pp. 63, 65, 68, 168.

<sup>46</sup> *August Comte and Positivism*, 63, 65, 68; *Works of J. S. Mill*, vol. i, 394.

<sup>47</sup> See, e. g., *Cours de philosophie positive*, vol. iii, 764, 765, 766, 789, 795, and 799; or see H. Martineau, *The Positive Philosophy of August Comte*, 1856, 381, 382, and 387f.



(1817-1878). While he acknowledges that Gall's localization of the organs of the particular functions was defective and that his anatomical data were inaccurate, he nevertheless maintains that Gall's hypothesis was scientific in character.<sup>48</sup> In his introduction to "Comte's Philosophy of the Sciences" he acknowledges his debt to Comte. Comte's influence, he says, "has been with me, surviving all changes of opinion, and modifying my whole mental history."<sup>49</sup> Lewes defines psychology as "the analysis and classification of the *sentient functions and faculties* revealed to observation and induction."<sup>50</sup> He draws for the first time a distinction between function and faculty. "By faculty is commonly understood the power or aptitude of an agent to perform a certain action or class of actions. It is thus synonymous with function, which means the activity of an organ, the uses of the instrument. I propose to detach faculty from this general signification, limiting it to the action or class of actions into which a function may be diversified by the education of experience. That is to say, let function stand for the *native* endowment of the organ, and faculty for its acquired variation of activity."<sup>51</sup> This very distinction, however, permits Lewes to mingle more inextricably than ever functions of mind, mental functions, and physiological functions. One would not speak readily, perhaps, of faculties of body or of bodily organs; but Lewes finds no difficulty in writing: "Instincts are functions. Emotions are functions. Sensation and perception are functions. Logical combinations are functions. Some functions are simple, others compound; that is to say, some are performed by single organs, as vision by the eye; others by groups of organs, as instincts and emotions."<sup>52</sup> "The mental functions are functions of the individual organism, the product, mind, is more than an individual product."<sup>53</sup> "To regard mind as a function of the organism, and yet suppose that some mental functions had no organic conditions, was a strange incongruity."<sup>54</sup>

Herbert Spencer (1820-1903) uses the word function very frequently. With regard to his usage he says: "In tracing out the genesis of nervous structures, a good deal has been implied respecting the genesis of accompanying functions.

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<sup>48</sup> *Comte's Philosophy of the Sciences*, 1853, 77.

<sup>49</sup> P. 2.

<sup>50</sup> G. H. Lewes, *Problems of Life and Mind*, 1879, Problem I, 6.

<sup>51</sup> *Op. cit.*, 27.

<sup>52</sup> *Loc. cit.*

<sup>53</sup> *Op. cit.*, 160.

<sup>54</sup> *Op. cit.*, 57.

. . . Throughout the foregoing argument, functions, when referred to, have been expressed in physiological language."<sup>55</sup> Spencer prefers to speak of activities or of faculties in connection with mind, and consistently refuses to use the term mental function. As is well known, he denies the doctrines of phrenology.<sup>56</sup>

In the works of Alexander Bain (1818-1904) the word 'function,' while not admittedly used in a systematic sense, as by Lewes, is nevertheless actually so employed. In the "Senses and the Intellect"<sup>57</sup> and in the "Emotions and the Will,"<sup>58</sup> besides using the word in the physiological sense, he frequently employs such expressions as "the thinking function of mind,"<sup>59</sup> "the most complicated of the mental functions, namely those related to Intelligence,"<sup>60</sup> "belief a voluntary function?"<sup>61</sup> In his "Mental Science" (1868) he ordinarily uses the word in the physiological sense, but we find instances of psychological usage: "The functions of Intellect, Intelligence, or Thought, are known by such names as Memory, Judgment, Abstraction, Reason, Imagination, . . . The real subdivisions of the intellectual functions . . . are (1) consciousness of *Difference*, (2) consciousness of *Agreement*, and (3) *Retentiveness*. Every properly intellectual function involves one or more of these attributes and nothing else."<sup>62</sup> Similar expressions are common in his "Logic" (1870).<sup>63</sup> In his critical notes in J. S. Mill's edition of James Mill's "Analysis of the Human Mind," he continually uses the term, and, with a single exception, in the psychological sense.<sup>64</sup> Bain was confessedly impressed and interested by phrenology. He tells us in his autobiography of controversial discussions of phrenology in which he took part in the Mechanics' Mutual Instruction Class which he joined at 17. Combe's "Constitution of Man," he says, "had a great influence at that time; and I think we went cordially along with it."<sup>65</sup> That his interest did not diminish in the following years, we are assured by his friendship for the

<sup>55</sup> *Principles of Psychology*, 1890, vol. i, 559.

<sup>56</sup> *Op. cit.*, 572ff.

<sup>57</sup> Published in 1855.

<sup>58</sup> Published in 1859.

<sup>59</sup> *Senses and the Intellect*, 1888, 321.

<sup>60</sup> *Op. cit.*, 45.

<sup>61</sup> *Emotions and the Will*, 1859, 604.

<sup>62</sup> *Mental Science*, 82.

<sup>63</sup> Edition of 1895, vol. i, 5, 8; vol. ii, 280.

<sup>64</sup> Vol. i, footnote, pp. 55-56, footnote, p. 228, footnote, p. 314; vol. ii, footnote, p. 181.

<sup>65</sup> Page 28.

phrenologist James Straton and interest in his measurements of heads. Bain wrote a number of articles appreciative of phrenology, for example, "Phrenology and Psychology,"<sup>66</sup> "The Propensities According to Phrenology Examined,"<sup>67</sup> "The Sentiments According to Phrenology Examined,"<sup>68</sup> "The Intellectual Faculties According to Phrenology Examined,"<sup>69</sup> "On the Study of Character, Including an Estimate of Phrenology,"<sup>70</sup> and the references in his writings<sup>71</sup> give further evidence of his regard for its doctrines.

Bain brings us to the time of William James (1843-1910) and to present-day usage, an account of which is given by C. A. Ruckmich in an article on "Use of the Term 'Function' in English Textbooks of Psychology."<sup>72</sup> James differentiates sharply between the structural and the functional aspects of mind.<sup>73</sup> It is under his influence that much of our modern 'function psychology' has arisen. James' sources are many, and the influences which bore upon him are difficult to evaluate. We know, however, that he studied and taught physiology for a number of years before he turned definitely to psychology; and phrenology and physiology in relation to consciousness come in for extended treatment in his "Principles." James criticizes phrenology, to be sure. "Modern science," he says, "conceives of the matter in a very different way."<sup>74</sup> "Large faculties and large 'bumps' might fail to coexist; . . . the scheme of Gall was so fast as hardly to admit of accurate determination at all . . . the whole analysis of the faculties was vague and erroneous from a psychologic point of view."<sup>75</sup> But, "there seems no doubt that Phrenology . . . may still be, in the hands of intelligent practitioners, a useful help in the art of reading character."<sup>76</sup> Compare with the last sentence, for example, Bain's statement that "the proper view to take of Phrenology is to regard it as a science of Character, accompanied with a theory of external indications."<sup>77</sup> Hodgson and Lotze, to

<sup>66</sup> *Fraser's Magazine*, May, 1860.

<sup>67</sup> *Op. cit.*, Sept., 1860.

<sup>68</sup> *Op. cit.*, Nov., 1860; Feb., 1861.

<sup>69</sup> *Op. cit.*, June, 1861.

<sup>70</sup> Published in 1861.

<sup>71</sup> *Mental Science*, 98; *Logic*, 287.

<sup>72</sup> *Loc. cit.*

<sup>73</sup> W. James, *Mind*, ix, 1884, 18f.; *The Principles of Psychology*, 1890, vol. i, 478.

<sup>74</sup> Vol. i, p. 29.

<sup>75</sup> Vol. i, p. 28.

<sup>76</sup> *Loc. cit.*

<sup>77</sup> *Logic*, 287.

both of whom James acknowledges indebtedness, use the term familiarly, and function was a popular concept with the French physiologists with whose works James must have been acquainted.

It seems clear enough, then, in conclusion, that *phrenology is the matrix from which our term is derived*. For we have seen that Gall, Spurzheim, and the other phrenologists use the word very often in the psychological sense, as the equivalent of faculty. Brown, the first English philosopher to use the term in this way, must have got it from the phrenological atmosphere, and not from older English philosophers or from his philosophical contemporaries. Later psychologists who were unacquainted with the doctrines of phrenology seldom use the word, and when they do so it rarely occurs in the psychological sense. Psychologists familiar with phrenology, on the other hand, disciple and foe alike, when speaking of phrenology, use the term in the psychological sense. The usage becomes more and more popular; so that in certain cases 'mental functions' are referred to quite systematically, especially where phrenological leanings are prominent, as in the cases of Lewes and Bain. By the time of James, the term has come into general use.

## THE THERMAL SENSITIVITY OF THE STOMACH<sup>1</sup>

By EDWIN G. BORING

The problem of the sensitivity of the stomach to thermal stimulation has never been settled.<sup>2</sup> Lennander denied all sensitivity to the stomach, and Müller failed to find thermal sensitivity. Weber, who could get only delayed coolness and warmth, was inclined to attribute the sensations to the skin of the abdomen. Hertz, Cook, and Schlesinger were unable to obtain temperature sensations from the stomach. They held that the apparent occurrence of such sensations with some subjects could not be due to conduction to the skin, but might be explained as the result of stimulation of the esophagus. In general, those who deny the thermal sensitivity of the stomach explain its apparent sensitivity under certain conditions as the result of stimulation (either by conduction or by chance contact) of the skin, of the body-wall, of the parietal peritoneum, or of the esophagus. Hertz states that the stomach is "rarely, if ever," sensitive to heat and cold. Meumann, on the other hand, insisted upon a general and well-differentiated stomachic sensibility. Becher, at first upholding the hypothesis of Lennander, reversed his judgment in regard to thermal sensitivity as the result of further experimental work. Quincke, Neumann, and Roux have all described cases of sensitivity to warmth and cold. It should be observed that even those writers who deny the sensitivity of the stomach to cold and hot stimuli find a 'warmth' or a 'burning sensation' which is elicited in the stomach by chemical stimuli. Meumann, Becher, Müller, and Hertz, Cook, and Schlesinger have observed this 'warmth.'

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<sup>1</sup> From the Psychological Laboratory, Cornell University.

<sup>2</sup> For discussions of conflicting results and for references to the names mentioned, see E. Becher, *Einige Bemerkungen über die Sensibilität der inneren Organe*, *Arch. f. d. ges. Psychol.*, 15, 1909, 361ff.; A. F. Hertz, F. Cook, and E. G. Schlesinger, *The Sensibility of the Stomach and Intestines in Man*, *Jour. Physiol.*, 37, 1908, 481ff.; A. F. Hertz, *The Sensibility of the Alimentary Canal*, 1911, 5ff.; and E. G. Boring, *The Sensations of the Alimentary Canal*, *Amer. Jour. Psychol.*, 26, 1915, 2ff.

The present writer has described experiments<sup>3</sup> in which warmth and cold were brought out by the introduction of warm and cold water into the stomach through a stomach-tube. The sensations came usually after a slight delay; the delay was less for the greater extremes of temperature; and the temperature of the water, when injected, had to be very much more extreme, if it was to produce sensation, than would a stimulus upon the outside of the body. These facts seemed to indicate that the sensory organs might be remote from the stomach and be stimulated through conduction. The qualitative identity of the cutaneous sensation and the sensation brought out by internal stimulation, if it does not positively support such a view, at least presents no objection to it. Accordingly the writer was inclined to accept the notion that these sensations do not originate in the stomach, although he promised at that time a further study. It was with this promise in mind that the experiments described below were undertaken. The fact that the conclusion on the basis of more recent work is in part a reversal of the writer's earlier belief is a demonstration of the value of the refinement of technique.

It was the object of these experiments to obtain a continuous record of temperatures within the stomach and to parallel that record by a contemporaneous report of sensation. It seemed that, if thermal sensations were aroused only after the conduction of heat from or to the stomach, it might be possible to arrange conditions so that the sensation would appear only *after* the most extreme temperature had been reached in the stomach. No such conditions were, as a matter of fact, found.

*Procedure and Apparatus.*—In all but two trials the writer (B) acted as subject. In two trials (W6, C6) Mr. F. L. Dimmick (D) was subject. In previous experiments<sup>4</sup> D had given evidence of being less sensitive to stomachic thermal stimulation. His observation here was intended as a check upon that of B.

As stimulus hot or cold water was introduced under air-pressure into the stomach through a stomach-tube. The apparatus has been described elsewhere.<sup>5</sup> The tube in the regular trials was swallowed until a mark 50 cm. from the end came opposite the teeth. In this position the end was undoubtedly well within the stomach. The lower end of the

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<sup>3</sup> *Op. cit.*, 4off.

<sup>4</sup> *Loc. cit.*

<sup>5</sup> *Loc. cit.*

esophagus in B is about 43 cm. from the teeth. The tube was not introduced further, because at 55 and 60 cm. it pressed against the stomach-walls sufficiently to cause discomfort and incipient vomiting. It was also found that in the lower positions the end could sometimes be felt pressed against the body-wall,—a degree of proximity to the superficial tissue that was considered undesirable in these experiments.

In addition to the preliminary trials, which were made with warm water and on B and which served to practise both observer and experimenters, there were seven trials with warm stimuli (W1 to W7) and seven with cold (C1 to C7). The warm stimulus, except in trial W7, was 25 cc. of water at 60° C. The cold stimulus, except in trial C7, was 25 cc. at 0° C. (Cf. Tables I and II.) In W7 and C7 less extreme temperatures (27° C for cold, 47° C for warmth) and greater amounts of water (75 cc.) were used. The first four trials of the W series were given successively, separated by short intervals, upon an empty stomach (3 hrs. after eating, 2 hrs. after drinking). The first four trials of the C series were given under like conditions on another day. Trials W5 and C5 with B, and W6 and C6 with D, were given together at another time. B's stomach was empty, D's only partially. W7 and C7 took place on an empty stomach at a fourth session.

Stomachic temperatures were measured by means of a thermocouple of copper and constantan. The wires to one junction were led through a small rubber tube, which in turn passed within the lumen of the stomach-tube. The water was blown through the stomach-tube, but outside the inner tube containing the wires. The thermo-electric junction was fastened flush with the end of the stomach-tube. The other junction was kept in a covered vessel of boiling water.<sup>6</sup> The difference of electric potential was read on a small d'Arsonval galvanometer with external scale and telescope.<sup>7</sup>

The record of every trial was made on a kymograph. An electric time-marker, connected with a metronome,<sup>8</sup> wrote seconds. In its circuit there was an electric bell, which was ordinarily short-circuited by a switch. The experimenter opened this switch during the time that

<sup>6</sup> Hence correction for barometer had to be made for the results of each session.

<sup>7</sup> The galvanometer was a cheap instrument and required adjustment before it could be relied upon. During the experiments its zero shifted by an amount equivalent to 2° C. A calibration taken after the series gave, however, a curve exactly parallel to the one taken before. Temperatures were therefore computed for each session with reference to the zero of the galvanometer for that session. Three calibrations taken at the same time gave a maximal deviation equivalent to 0.4° C.

<sup>8</sup> The metronome showed a variable error of 1.5% and a constant error of 2.9% in 10 trials of 100 secs. each. Results were corrected for the constant error.

he was squeezing the bulb which forced the water into the stomach. The intermittent current, which resulted from placing the bell in circuit, made the time-marker write an alternately broad and narrow line, which indicated, besides the seconds, the duration of the time required to introduce the stimulus. A second signal-magnet was connected with two keys under the control of the person who read the galvanometer. The one key caused a simple depression of the signal; the other, connected through a second electric bell, made the marker vibrate so as to write a broad line. The experimenter, set to read the galvanometer, signalled with the second key whenever the cross-hairs of the telescope crossed one of the main scale divisions (*i. e.*, one of those five divisions apart). He signalled with the other key for the intermediate divisions, except when the change was too rapid for him to catch all the divisions; in such places the intermediate divisions were left out. A third line was written by a tambour, connected with a rubber bulb which the observer squeezed to indicate when he felt temperature sensation.<sup>9</sup>

Two experimenters were required, one to manipulate the apparatus and one to read the galvanometer.

Several possible sources of error should be noted. (1) It was not possible to use a double-walled stomach-tube, since the lumen had to be large enough to contain the tube through which the wires ran and still to allow space for the water to pass.<sup>10</sup> The danger arising from the lack of this precaution is not as great as might appear, because it has already been shown that the thermal sensations, which it is the object of this study to explain, occur when a double-walled tube is used. In general the 'stomachic' warmth and cold are referred lower down than the esophageal, although they may also spread to the throat and chest;<sup>11</sup> and the sensations reported in these trials showed this same stomachic pattern of reference. (2) A variable error of the galvanometer has been mentioned. Readings should, perhaps, not be considered as accurately indicating absolute temperature to less than 0.4°C. (3) The errors of the metronome are negligible, especially as the absolute times are of little importance. (4) The reaction times of the experimenter who read the galvanometer are involved in the results. It is impossible to say how accurate he was. Doubtless his error is considerable in the cases where the temperature was changing very rapidly just at the introduction of the stimulus. The large M.V. for the temperature at which sensation first appears (p. 493) indicates that these early temperatures are less accurate. The error should be negligible for the gradual change in the later parts of a trial.

*Results.*—In order to render the results comparable, temperature curves were platted between time and temperature

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<sup>9</sup> In the first experiments this same tambour was used to indicate the duration of the giving of the stimulus. Sensation, however, appeared so soon that the two curves were apt to interfere; hence the method described above was adopted.

<sup>10</sup> The actual tube used was the stethoscope tubing of Tube No. 3, *op. cit.*, p. 6; outside diam. 9 mm.; lumen, 5 mm. The wires ran through a tube 4 mm. outside diam.

<sup>11</sup> *Op. cit.*, 20, 42.



for every trial.<sup>12</sup> The observed points, except where some readings were not recorded during the rapid change of temperature in the first few seconds, were about one-half of a degree apart. The interpolated temperatures must then, except where the change was rapid, have been sufficiently accurate, perhaps to 0.1° C. In Tables I and II will be found the temperatures of the stomachic contents as read off from the platted curves at intervals of two seconds. Those temperatures at which there was a report of warmth or cold (as written by the tambour) are printed in heavy-faced type. In some cases, it will be observed, the thermal sensation disappeared for a few seconds and then reappeared again. Nearly all the trials ran beyond 40 secs., sometimes to 60 or 70. No reports of sensation, except in one preliminary trial, occurred so late.

TABLE I

STOMACHIC WARMTH. Figures show variation of stomachic temperature (°C) at different times (secs.) after beginning of introduction of stimulus. **Heavy-faced type** indicates temperatures at which a thermal sensation was felt. End of tube, 50 cm. from teeth.

No. Trial:.....	W1	W2	W3	W4	W5	W6	W7
Observer:.....	B	B	B	B	B	D	B
Amt. stim.: cc.....	25	25	25	25	25	25	75
Temp. stim.: °C.....	60	60	60	60	60	60	47
Seconds	Temperature: °C.						
0.....	37.0	37.0	37.0	37.0	37.0	37.0	37.0
2.....	37.0	37.0	37.0	37.1	37.0	37.0	37.7
4.....	37.0	37.2	37.4	<b>37.5</b>	37.5	37.6	38.2
6.....	<b>41.9</b>	41.8	<b>41.0</b>	<b>41.5</b>	<b>39.0</b>	<b>38.9</b>	39.5
8.....	<b>42.0</b>	<b>42.2</b>	<b>42.4</b>	<b>42.2</b>	<b>40.1</b>	<b>39.2</b>	<b>40.6</b>
10.....	<b>42.0</b>	<b>42.5</b>	<b>42.9</b>	<b>42.6</b>	<b>40.4</b>	<b>39.6</b>	<b>41.1</b>
12.....	<b>41.9</b>	<b>42.6</b>	<b>43.0</b>	<b>42.8</b>	<b>40.5</b>	<b>39.7</b>	<b>41.4</b>
14.....	<b>41.9</b>	<b>42.5</b>	<b>42.8</b>	<b>42.8</b>	<b>40.5</b>	<b>39.6</b>	<b>41.5</b>
16.....	<b>41.9</b>	<b>42.3</b>	<b>42.1</b>	<b>41.8</b>	<b>40.5</b>	<b>39.2</b>	<b>41.5</b>
18.....	<b>41.9</b>	<b>42.2</b>	41.5	<b>41.3</b>	<b>40.5</b>	<b>39.0</b>	<b>41.5</b>
20.....	<b>41.9</b>	<b>42.1</b>	41.0	<b>41.1</b>	<b>40.5</b>	38.8	<b>41.5</b>
22.....	<b>41.8</b>	<b>42.0</b>	<b>40.6</b>	<b>40.9</b>	40.4	38.7	<b>41.3</b>
24.....	<b>41.7</b>	<b>41.8</b>	<b>40.4</b>	40.6	40.4	38.6	<b>41.1</b>
26.....	41.5	<b>41.7</b>	<b>40.3</b>	40.4	40.4	38.5	40.9
28.....	41.2	<b>41.5</b>	<b>40.2</b>	40.2	40.3	38.4	40.7
30.....	40.9	41.4	<b>40.1</b>	40.0	40.2	38.3	40.5
35.....	<b>40.5</b>	41.1	39.9	39.7	40.1	38.1	40.3
40.....	40.1	40.9	39.8	39.4	39.9	38.0	40.1

<sup>12</sup> Fairly smooth curves were drawn through all points. These curves departed appreciably from the straight lines connecting the points only during the first few (6-10) secs. The only wide departure allowed here was in conformity with the shape of all the other curves in which intermediate points were recorded.

TABLE II

STOMACHIC COLD. For interpretation, see Table 1.

No. Trial:.....	C1	C2	C3	C4	C5	C6	C7
Observer:.....	B	B	B	B	B	D	B
Amt. stim.: cc.....	25	25	25	25	25	25	75
Temp. stim.: °C.....	0	0	0	0	0	0	27
Seconds	Temperature: °C.						
0.....	37.0	37.0	37.0	37.0	37.0	37.0	37.0
2.....	37.0	37.0	37.0	36.9	36.8	36.8	37.0
4.....	36.8	35.4	35.5	35.0	35.3	33.6	33.2
6.....	34.7	27.7	27.4	27.0	28.0	29.2	30.7
8.....	24.9	23.3	22.7	22.9	24.9	29.0	29.6
10.....	23.7	19.5	20.1	22.3	23.8	28.8	29.5
12.....	23.1	19.8	19.3	22.2	23.2	28.6	29.7
14.....	22.8	20.4	19.4	23.0	23.2	28.5	29.9
16.....	22.8	21.0	19.6	23.8	23.6	28.4	30.0
18.....	23.0	21.6	20.3	24.3	24.0	28.6	30.2
20.....	23.4	22.1	21.2	24.8	24.6	28.8	30.4
22.....	23.7	22.6	22.0	25.3	25.1	29.6	30.6
24.....	24.1	23.0	22.9	25.8	25.6	30.4	30.8
26.....	24.4	23.4	23.6	26.2	26.0	30.6	31.0
28.....	24.8	23.8	24.1	26.5	26.4	31.0	31.2
30.....	25.2	24.2	24.6	26.8	26.7	31.9	31.3
35.....	26.0	25.3	25.3	27.5	27.5	34.6	31.5
40.....	26.6	26.2	26.0	28.2	28.1	35.4	31.8

*Reports.*—The observer's localizations and reports upon quality show that the spatial-qualitative patterns are identical with those previously found with the double-walled tube.<sup>13</sup> Both cold and warmth tend to spread up toward the throat in a diffuse pattern like that of esophageal temperature; but there is added a concrete, definitely localized, thermal sensation, lower down than the esophageal complex extends.<sup>14</sup> In three out of four cases when a recurrence of the thermal sensations was recorded the second sensation was referred farther down, an observation which suggested that localization might indicate the course of the stimulus to the pyloric end of the stomach.<sup>15</sup>

This hypothesis was supported by the results of a few trials. Twenty-five cc. of water at 60° C were introduced through a double-walled tube three times with the end of the tube at 40 cm. from the teeth (lower end of the esophagus), three

<sup>13</sup> *Op. cit.*, 42f.

<sup>14</sup> *Op. cit.*; compare Figg. 4-8, 20-24, 36-50 (p. 20) with Figg. 109-112, 117-120 (p. 42).

<sup>15</sup> A striking illustration of the shift of reference of a stomachic warmth in accordance with the position of the stomach is shown in Fig. 121, *op. cit.*, 42.

times at 50 cm. (cardiac end of stomach), and three times at 60 cm. (pyloric end,—unless the tube gets unusually kinked in the stomach). The observer (B) localized the lowest point of reference of the resultant warmth. The localizations (in terms of a centimeter scale of rectilinear body-coördinates in which "40," *e. g.*, is a point on the surface of the body directly over a point in the esophagus which is 40 cm., measured along a stomach-tube, from the teeth<sup>16</sup>) are as follows:

Distance of end of tube from teeth.....	40 cm.	50 cm.	60 cm.
Av. longitudinal localization.....	42.8 ±2.5	51.3 ±3.4	56.2 ±5.3
Av. transverse displacement.....	0 ±0	Left, 1.1 ±0.4	Right, 2.2 ±3.7

A larger number of observations would give more reliable results. The indication is definite, however, that localization—at least with a practised observer—may serve as a basis for differentiation of stomachic from esophageal warmth.

*Conduction Through the Body-Wall.*—In order to discover whether the introduction of water at extreme temperatures has any sensible effect upon the temperature of the abdominal skin a trial was arranged in which a thermometer, reading to  $0.1^{\circ}$  C, was bandaged against the skin. The bulb, 3 cm. long, was placed on the median line of the body from the points 48 to 51 (scale of body-coördinates). It was covered by a piece of felt, 10 cm. square, over which was placed a large cotton pad. A heavy bandage, 25 cm. wide, was then wrapped tightly six times around the body. The thermometer extended from underneath the bandage so that the part of the scale showing body-temperature could be read. The readings could be made accurately to less than a scale division by an experimenter who, at the moment of reading, held a pocket flash-light behind the level of the top of the column. As the bandages warmed up the mercury rose. Finally it recorded  $36.9^{\circ}$  C. It had taken over five minutes to rise the last  $0.1^{\circ}$ . At this point a second experimenter pumped 500 cc. of water at approximately  $0^{\circ}$  C into the stomach through a stomach-tube which extended 50 cm. from the teeth. It required at least a minute to get the water in. About midway in the course of the introduction of the stimulus the mercury slowly began to fall and continued until it reached  $36.85^{\circ}$ , where it remained until some time after the cold water had all been introduced. Then it gradually rose again to  $36.9^{\circ}$ .

<sup>16</sup> Cf. Plate II, *op. cit.*, opp. p. 12.

The warming of 500 cc. of ice water to body-temperature (18,500 calories) had produced a maximal change of temperature in an optimally situated point on the skin of  $0.05^{\circ}\text{C}$ .<sup>17</sup>

*Conclusions.*—We have seen that the sensations arising upon thermal stimulation of the stomach have been attributed by different writers to the body-wall, to the esophagus, or to the stomach itself.

1. *The case against the body-wall.*

a. The sensations can not come from the abdominal skin. If 500 cc. of water at  $0^{\circ}\text{C}$  produce only  $0.05^{\circ}$  drop in the skin, it is out of the question that 25 cc. at  $27^{\circ}$  should arouse cutaneous sensation.<sup>18</sup> Just how deep the organs must lie it is difficult to say. One can only guess at the curve of transfer of heat for living tissue, in which the tendency of the circulating blood is to keep the temperature constant. In general one would expect that the effect of a thermal stimulus would not be very far-reaching, and the very small effect that we have found at the surface of the body supports such a view. The organs are probably not only not in the skin, but also very much more immediate to the stomach than they are to the superficial dermal layers.

b. The argument that the temperature-organs are stimulated only by conduction from the stomach, because the adequate temperatures are extreme and because sensation occurs after a latent time, is not borne out by fact. The adequate stimulus within the stomach may be less than five degrees above or ten degrees below body-temperature (p. 493). The water stimulus, in passing through the tube, in mixing with the stomachic contents (there is possibly always some content), and in getting into intimate contact with the walls of the stomach, approaches more nearly to the temperature of the body than had been supposed. The interpolation of these same factors also accounts for the apparent latent time, which is really a time that elapses before the stimulus has come intimately into contact with the gastric tissues.

2. *The case against the esophagus.*

a. The introspections show in general that the spatial-qualitative pattern is like the stomachic patterns obtained with a

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<sup>17</sup> Previous trials with the thermo-couple bandaged against the skin had given negative results; but the galvanometer-scale could not easily be read to less than  $0.1^{\circ}\text{C}$ . Dr. S. I. Franz suggested the use of the thermometer.

<sup>18</sup> Hertz, Cook, and Schlesinger have argued against the origination of these sensations in the body-wall. *Op. cit.*, 484.

double-walled tube and unlike the esophageal patterns obtained in that manner. On the general basis of the identical character of the experiences we are entitled to assume that the results with the single-walled tube are as valid indicators of true stomachic sensation as are the previous results with the double-walled tube.

b. More specifically we have reason to believe that the localization of warmth or cold at a point considerably below the average lowest point for the corresponding esophageal sensation is a fairly reliable indicator that warmth or cold arises, not in the esophagus, but in an inferior region.

If neither the more superficial abdominal tissue nor the esophagus mediates these sensations, we must conclude that *the warmth and cold which arise from thermal stimulation of the stomach originate in organs lying either in the tissues of the stomach itself or in regions nearer to the stomach than the outer layers of the body-wall.* In this sense we may speak of 'stomachic warmth' or of 'stomachic cold.'

*Degree of Sensitivity of the Stomach.*—It should be possible to make some statement with regard to the stimulus-limens of stomachic warmth and cold. Below are given average values—each one is a mean of all seven trials—for (a) the temperature at which the thermal sensation was first noted; (b) the temperature at which, after being noted, the sensation first disappeared; (c) the temperature, in the case of a recurrence after a first disappearance, at which the temperature last disappeared; and (d) the least extreme temperature at which the sensation was noted in the given trial.<sup>19</sup>

	(a) First Appearance	(b) First Disappearance	(c) Last Disappearance	(d) Least Extreme
Warmth....	39.7 ±1.04	40.9 ± .73	40.4 ± .43	39.6 ± .91
Cold.....	29.5 ±4.21	25.7 ±2.73	26.1 ±2.49	30.0 3.69

Any one of these values might be taken as a limen. The (a) figures are subject to the error incurred in reading the galvanometer-scale when it was changing very rapidly. The large M. V., which is larger with the rapid change for cold than with the slower change for warmth, supports this view. The figures under (c) include temperatures taken at the cardiac end of the stomach for sensations felt at the pyloric

<sup>19</sup> The values are taken from the platted curves, not from the selected values of Tables I and II.

end. We should not be wise to accept figures which are known to apply to different conditions. The values given last, (d), include both the errors just mentioned. Nevertheless we can not take the results under (b) as representative, because in these there has been time for adaptation to set in. That adaptation may be very rapidly effective is suggested by the fact that the average value for the first disappearance of cold is  $25.7^{\circ}$  C, whereas in a single case (C7) cold was elicited by water which was only  $27^{\circ}$  before introduction and which brought the stomach contents down only to  $29.5^{\circ}$ . We may best suspend judgment, with the general statement that stomachic warmth may be brought out by a stimulus of about  $40^{\circ}$  C and stomachic cold by a stimulus of about  $30^{\circ}$  C.<sup>20</sup>

*Summary.*—A sensation of warmth is produced by a stimulus applied to the stomach at approximately  $40^{\circ}$  C; a sensation of cold is produced by a stimulus applied at approximately  $30^{\circ}$  C.

These thermal sensations arise either in the stomach itself or in tissues more immediately adjacent to the stomach than are the outer abdominal wall and the esophagus.

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<sup>20</sup> The contention of Head that "epicritic" sensibility is not possessed by the viscera would seem, on the basis of these results, to find an exception in the case of the stomach.

# THE SELF IN SCIENTIFIC PSYCHOLOGY

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## A. INTRODUCTION

The self is often bowed out of psychology on the ground that scientific introspection has failed to discover it. The object of this paper is to examine and to estimate this charge. The problem is two-fold. First, is the fact as stated: have scientific psychologists really found no trace of a self? Second, if the fact be admitted, is the failure to produce a self due to the inadequacy of the methods or to the non-existence of the self? (Is there no fox at all or does he avoid the traps we have set for him?) The discussion of these main questions will follow upon an introductory section which considers the nature of scientific method and the meaning given to the term 'self' by those who claim it for psychology. These preliminary topics will be discussed in reversed order.

### I. *The Nature of the Psychologist's Self*

The self is indefinable. To define is to assign the object defined to a given class and to distinguish it from other members of the class; and the self is *sui generis* and therefore incapable of definition. To quote Oesterreich, it "is a kind of thing which one can merely indicate (*auf das man nur hinweisen kann*) but which one can as little demonstrate to the I-blind as one can demonstrate color to the color-blind."<sup>1</sup> But Oesterreich's simile must not mislead us. The self, though indefinable, is not on this account elemental and thus indescribable. Its characters, as Miss Gamble points out, are 'properties' not 'differentiae.'<sup>2</sup> The characters of the experienced self on which the self-psychologists lay their emphasis are, first, its persistence or self-identity; second, its individuality or uniqueness; third, the fact that it is fundamental or basal to its experiences, and finally the fact that

<sup>1</sup> K. Oesterreich, *Die Phänomenologie des Ich*, 1910, p. 197<sup>1</sup>.

<sup>2</sup> E. A. McC. Gamble, "A Defence of Psychology as Science of Selves," *Psychological Bulletin*, 1915, XII., p. 197<sup>1</sup>.

it is related to its environment, social and physical. These characters have elsewhere been discussed by the writer and will be very briefly stated.<sup>3</sup>

(1) From Stuart Mill, in his well-known Note on James Mill's "Analysis of the Phenomena of the Human Mind,"<sup>4</sup> to Knight Dunlap, with his 'notion of the Ego as an essential presupposition of psychology,'<sup>5</sup> everybody who admits the self at all credits it with relative persistence, or identity. "Suppose," Dunlap says, "three items of content,—*a*, *b*, and *c*. Suppose I am aware of *a*, then of *b*, then of *c*. . . . The fact that I perceive all three . . . remains to the end an ultimate fact. The important thing is that the three items . . . are perceived by the same I. The perceptions are not the same; they may be separated by considerable intervals. What is the identity? Merely the identity of the I."<sup>6</sup>

(2) The individuality, or uniqueness of each self, is the character which distinguishes it from every other: all self psychologists, and often even those who deny the self, agree that "psychic facts belong to individuals" that "a feeling is either mine or somebody else's."<sup>7</sup>

(3) Both the persistence and the individuality of the self imply the fact that it is basal to the specific, concrete experiences—to the perceivings, imaginings, and emotions. In the words of Oesterreich, "all genuine psychic processes [are] states or functions of a subject, belong to an I."<sup>8</sup> Or, in Dunlap's simpler statement, "we cannot talk of experiencing without an I which experiences."<sup>9</sup> The self to be sure is never divorced from the specific experiences; as Oesterreich says, "it is not a something existing for itself beyond or beside" the experiences. On the contrary, the experiences

<sup>3</sup> For further consideration of the characters of self, cf. the writer's "A First Book in Psychology," 4th edition, 1913, pp. 3 *et al.*, and XIII, and "Psychology: what is it about," *Journal of Philosophy*, 1908, V., pp. 65-67. Cf., also, A. Pfänder, *Einführung in die Psychologie*, esp. pp. 374, 380. For criticism of this view, cf. J. N. Curtis, this JOURNAL, 1915, 26, pp. 782-851.

<sup>4</sup> Note 33 to Vol. II., chapter XIV., Section 7.

<sup>5</sup> "The Self and The Ego," *Psychological Review*, 1914, XXI., p. 622. Cf. Dunlap, "A System of Psychology," chapters XVI and XX.

<sup>6</sup> *Psychological Review*, *op. cit.*, XXI., pp. 66-67.

<sup>7</sup> S. Witasek (an idea-psychologist), "Grundlinien der Psychologie," 1908, I Teil, Kap. 2, p. 38. Cf. James, "Psychology, Brief Course," 1892, p. 153; and Knight Dunlap, "The Self and The Ego," *Psychological Review*, 1914, XXI., p. 664.

<sup>8</sup> "Die Phänomenologie des Ich," 1910, p. 225.

<sup>9</sup> *Op. cit.*, p. 682.



exist in the self; in Ach's phrase, it constitutes their 'essential foundation.'<sup>10</sup>

(4) The relatedness of the self to the objects making up its environment has been specially stressed by the 'social' and 'differential' psychologists,<sup>11</sup> by Ward,<sup>12</sup> Mitchell<sup>13</sup> and Rehmke,<sup>14</sup> and by those 'functional' psychologists—notably Angell and Judd—who admit a self. "The standpoint of psychology" as Ward says "is that of the living subject in intercourse with his special environment."

In conclusion it should be stated explicitly that no finality is claimed for the enumeration of these characters as given in the pages preceding. The psychological description of self needs to be clarified, widened, and enriched by the efforts of all psychologists working in all branches of the science.<sup>15</sup>

## II. Psychological Methods

Up to this point, only the first of the terms of our problem has been considered. From the formulation of the meaning of 'self' we turn, therefore, to a discussion of the nature of psychological method. Like the other sciences psychology may be said to employ two methods—observation and experiment. The first-named is fundamental since experiment always involves observation. Scientific observation is distinguished from the every-day consciousness of objects first, because it analyzes the objects which the plain man sees *en bloc* and as wholes; second, because it is always supplemented by classification of the observed objects. Experiment, as distinguished from observation, has the two-fold purpose of aiding analysis and of explaining the observed facts by dis-

<sup>10</sup> N. Ach, "Über den Willensakt und das Temperament," 1910, pp. 248<sup>2</sup>-249.

<sup>11</sup> Cf. E. A. Ross, "Social Psychology," pp. 26f, 43f, 326 *et al.*; and W. Stern, "Die differentielle Psychologie," pp. 30, 57, 320f. *et al.*; and J. M. Baldwin, "Social and Ethical Interpretations." Part I.

<sup>12</sup> "On the Definition of Psychology," *British Journ. of Psychol.*, 1904, I., pp. 1ff.

<sup>13</sup> W. Mitchell, "Structure and Growth of the Mind," Lect. I., p. 11 *et al.*

<sup>14</sup> Lehrbuch der allgemeinen Psychologie," Iter Teil, esp. §§ 11, 12. Cf. Angell, "Psychology," p. 7; K. Dunlap, *op. cit.*, pp. 63, 68<sup>2</sup>.

<sup>15</sup> Dr. Josephine N. Curtis, in the paper cited on p. 496 above, makes the criticism that all the characters of the self should be subsumed under that of 'relatedness.' To consider the interesting suggestion involved in this criticism would be beside the purpose of this discussion which is concerned merely to note that the self has these four characters whether or not they are strictly coördinated. It should be observed also that the relatedness, here emphasized, is that of the self to its objects, or environment.

closing their relation to each other or to still other phenomena. The remainder of this section will discuss these methods as applied in psychology—first, experiment; and second, the fundamental method, observation, or, as used in psychology, introspection.

### *a. The method of experiment*

In order to decide whether experimental psychology is adapted to the discovery of a self it is essential to keep in mind the nature of experiment. Roughly speaking, experiment consists in the artificial manipulation of one's material in such wise as to repeat, to isolate, and to vary it at will. Rigid experiment, as carried on by physical scientists, demands also measurement of the phenomena that they may be exactly repeated and varied.

It is obvious from this statement that, in the strictest sense of the word, experiment is impossible to psychology since psychic facts, like other phenomena of life, can be neither repeated and varied at will nor accurately measured. On the other hand, the physical conditions of perception—and indirectly of the predominantly sensational experiences, memory and imagination—can be isolated, accurately measured, repeated, and varied. And the motor reactions due to changes in either physical or psychical conditions can be measured, though they can not be exactly varied at will.

In a more or less modified sense, therefore, psychology may be called experimental in so far as it deals with perceptual experience or with motor reactions conditioning, accompanying, or following on consciousness. But this widening of the concept of experiment offers little hope of discovering the self through experiment. For, as regards experiments on perception and the allied forms of consciousness, self-psychologists teach either that it is difficult or that it is utterly impossible to observe the self in the perceptual experience. And the study of motor reactions, however important, is a subsidiary part of psychology.

The inevitable presence of the self in all experience constitutes another reason why the thorough-going self-psychologist should question his right to lay claim to the experimental method for the discovery of the self. This difficulty may be stated as follows: A phenomenon under experimental investigation may be regarded as the effect of its artificially manipulated stimulus. An experiment thus conceived should be judged by its conformity with the acknowledged inductive

methods. Under the given conditions, in an experimental study of any phenomenon, as X, one must study first, repeated phenomena which include X, and second, a series of phenomena resembling the first group except as they exclude X; and only in case of confirmatory results from both parts of the study has one a strictly valid experimental result.<sup>16</sup> But this exclusion of X, when X is the self, can obviously never be secured if whenever I am conscious I am conscious of self. And so the omnipresence of the self disqualifies it as object of experiment.<sup>17</sup>

The conclusions from this study of experiment as a psychological method are then the following: Experiment is possible only in a modification of the physicist's sense of the word to psychology as a whole. And psychological experiment is, for two reasons ill adapted to the investigation of self. Because self is 'ubiquitous,'<sup>18</sup> we can not test its presence under rigidly verifiable experimental conditions. And because the self is unemphasized in perception and imagination we are unlikely to find it by the modified experimental method which must limit itself mainly to the field of sensational experience.

### *b. The method of introspection*

From ordinary scientific observation introspection is distinguished solely by its object which is a psychic not a physical nor a logical object. To quote Titchener, who more strongly than any other writer urges this truth, introspection "is in all essentials identical with the observational procedure of the natural sciences;"<sup>19</sup> "there is absolutely no difference in principle between introspection and inspection."<sup>20</sup>

Introspection as a method of scientific psychology must be carefully differentiated from the unscientific variety of self-observation. Here, again, one can do no better than to follow rather closely Titchener's lead. Two sharp distinctions must be made. (1) Scientific introspection differs in the first place from "a rationalizing philosophy" which reflectively interprets consciousness "in terms of some phil-

<sup>16</sup> Cf. J. S. Mill, "A System of Logic," Book III., Chapter VIII., § 4.

<sup>17</sup> Those self-psychologists who teach that we are often, but not invariably, conscious of self will not be affected by this consideration. Cf. pp. 517f., below.

<sup>18</sup> For use of this term consult R. B. Perry, "The Egocentric Predicament," *Journal of Philosophy*, 1910, VII.

<sup>19</sup> The Schema of Introspection," this JOURNAL, 1912, 23, p. 487<sup>1</sup>.

<sup>20</sup> "Experimental Psychology of the Thought Processes," p. 238<sup>1</sup>.

osophical system."<sup>21</sup> The interpretation may be animistic, or materialistic, or idealistic; but in so far as a writer discusses what must be inferred to exist, not what is or has been directly experienced, he is not simply introspecting.

(2) Introspection, in the second place, is distinguished both by its method and by its aim, from every-day, 'common-sense' self-observation. The plain man's observation of self, like his observation of any object, is unanalytic. It deals with experiences as wholes instead of analyzing out and emphasizing the peculiarities marking off one experience from another. The terms which record these untechnical introspections are an evidence of their unanalytic character. When "a half-trained observer," once more to quote Titchener,<sup>22</sup> records the experience of 'puzzle' or 'perplexity,' the word tells the teacher, "nothing whatsoever of the observer's individual experience, of the particular 'feels' that constituted the perplexity in the particular case."

In purpose as well as in method the psychologist and the unscientific introspector differ. The former has a theoretical aim: to understand consciousness. The latter regards his experience from the practical, not from the theoretical, scientific point of view. The unanalytical terms of his self-observation serve him simply and amply as cues to action. Nothing more is necessary "for the affairs of every-day life, for social intercourse, for the regulation of behavior."

One particular sub-form of every-day self observation should be distinguished with special care from scientific introspection. It may be described as valuing self-observation, and includes the morbid, the sentimental, and the ethical varieties of attention to consciousness. Titchener has it in mind in his distinction of 'the introspection of the psychological laboratory' from that of a 'moralising common-sense;' but the term 'moralising' is too narrow to be used, as he here uses it, to cover the "absorption, anxious or complacent, in the strength of our intellect, the delicacy of our sentiments, the firmness of our resolution." The psychologist, in contrast with the introspector of this type, observes himself, as Titchener truly notes, not for any motive of self-interest or self-appraisal or self-glorification but "because his mind is the only mind directly accessible to him, and mind is the topic

<sup>21</sup> E. B. Titchener, "Prolegomena to a Study of Introspection," this JOURNAL, 1912, 23, p. 436.

<sup>22</sup> "Description vs. Statement of Meaning," this JOURNAL, 1912, 23, pp. 167<sup>2</sup>f.

of his professional interest."<sup>23</sup> Titchener adds that "introspection may be as impersonal, as objective, as matter of fact as is the observation of the natural sciences;" and the self-psychologist gladly subscribes to this statement, on the understanding that 'impersonal' means 'disinterested.'

From discussion of the nature of introspection the transition is easy to the consideration of its different forms. For the purposes of this paper, the distinction between direct and indirect introspection need be only named.<sup>24</sup> Some psychologists, notably Ach, contend that direct introspection, involving a distraction of attention from the 'object' to the 'subject' of consciousness is rarely if ever, possible. The present writer however finds, with other observers, that it is sometimes, at least, possible to attend to attention<sup>25</sup> without dispersing it and agrees with Titchener that the disturbance which Ach notes is due rather to the attempt to record introspection than to introspection itself. "The fact is simply" Titchener says,<sup>26</sup> "that when an experience is in process you cannot . . . take note of it, find forms of verbal expression for it, report upon it; the experience will not wait for you."

It is of far more importance to distinguish introspections according to their greater or less approximation to experimental procedure. To draw hard and fast lines of division would be as futile as it would be difficult, but it seems possible, from this point of view, to distinguish at least two stages of introspection:—

I. Incidental (or, in Titchener's phrase,<sup>27</sup> 'casual') scientific introspection under 'standard conditions.' Of course, much of our incidental introspection is not scientific but in "remembering green" for example, we can "(1) keep distracting stimuli away, and (2) introspect the memory-green or the fancy-

<sup>23</sup> "Prolegomena to a Study of Introspection," this JOURNAL, 1912, 23, pp. 433-434<sup>1</sup>.

<sup>24</sup> Cf. Titchener, "The Schema of Introspection," this JOURNAL, 1912, 23, pp. 491-493 with footnote 11, 502-503 with footnote 45; and "Experimental Psychology of the Thought-Processes," pp. 237ff. These passages discuss also the difference between Ach and other experimental introspecters in regard to the relative advantage of introspection guided by questions from the experimenter, as compared with unguided introspection. But this consideration, also, is not relevant to the main purpose of this paper.

<sup>25</sup> The self-psychologist would describe such introspection as attention to the self-attending-to-its-object. From this point of view, introspection (as secondary attention) has a two-fold object—(1) the attending self, (2) the object of the primary attention.

<sup>26</sup> Cf. "Experimental Psychology of the Thought-Processes," p. 237.

<sup>27</sup> "Prolegomena to A Study of Introspection," this JOURNAL, 1912, 23, p. 444<sup>3</sup>.

green in an even frame of mind. These are standard conditions. They can be accurately recorded by the psychologist who introspects and they can be repeated by other psychologists."<sup>28</sup> Any experience, whatever the circumstances under which it occurs, can be scientifically introspected so far as distraction is eliminated, an 'even frame of mind' is preserved, and accurate record is made.

2. From such incidental scientific observation, 'systematic' or 'controlled,' introspection is distinguished in that the introspection is deliberately made, often in the interest of a specific theoretic problem, and follows upon pre-arranged signals, verbal or concrete, to one or another sort of mental operation. There are two sub-forms, of systematic introspection. In the first, and simpler, the pre-determined stimuli to introspection<sup>29</sup> are incapable of measurement and of any save inexact variation. Accordingly, only introspective reports, with no 'objective' results as by-products, are gained by this procedure. Bühler's use of the question-method is a classic example. Beyond the general qualification of their fitness to stimulate thought there is no essential likeness between the problems set to Bühler's subjects, which vary from such questions as, "Can we by our thinking apprehend the nature of thought?" to such others as, "Can you reach Berlin in seven hours?"<sup>30</sup>

The second form of systematic introspection is distinguished by the experimental control of the physical conditions of the introspection. There are again two sub-classes.<sup>31</sup> In the

<sup>28</sup> In "A Primer of Psychology," pp. 33-35, from which this quotation is made, and occasionally in his later writings, Titchener designates such an observation as 'experimental introspection.' The writer, as will appear, prefers to give a narrower meaning to this phrase. It should, however, be observed that incidental, scientific introspection shades almost imperceptibly into the non-experimental form of systematic introspection.

<sup>29</sup> The expression 'stimuli' (more accurately 'physical stimuli') is used here, and in the pages which follow, in untechnical sense to apply to any non-psychical incitements to consciousness—such as words, diagrams, pictures, clicks.

<sup>30</sup> K. Bühler, "Tatsachen und Probleme zu einer Psychologie der Denkvorgänge," *Archiv. f. d. ges. Psych.* 1907, IX., p. 304.

<sup>31</sup> This classification of the forms of introspection may be summarized as follows:

- I. Incidental Introspection
- II. Systematic Introspection
  - a. Non-experimental
  - b. Experimental:

1. To predominantly sensational experiences.
2. To predominantly unsensational experiences.

first of these—the experimental introspection of sensation, perception, memory, and imagination—the conditions of the experience can be wholly, or largely, controlled. In the second—the experimental introspection of thought, recognition, emotion, and will—the physical stimuli constitute a small part only of the antecedent conditions, for these ‘higher processes,’ in far greater degree than perception and imagination, are psychically conditioned. Yet, as part-conditions, the physical stimuli can—with advantage to psychological analysis—be repeated and, to greater or less degree, varied and measured. Such a procedure will yield not only records of introspection but also material for ‘objective treatment.’ Examples of these reproducible stimuli to non-perceptual consciousness are the diagrams, some repeated and some varying more or less from the standard, which Katzaroff showed to his subjects in his study of recognition;<sup>32</sup> the numerals presented by Michotte and Prüm in their study of choice;<sup>33</sup> and the series of nonsense-syllables learned, by the subjects of Ach’s study of will, through varying numbers of repetitions.<sup>34</sup>

In comparing these forms of scientific observation it must be borne in mind that all forms, even incidental introspection, can secure the ‘general conditions of psychological experiment,’ the physical comfort, the trained attention, and the impartiality of the observer. Systematic introspection, even in its non-experimental form, may have two further advantages: first, and most important, the coöperation of laboratory-trained subjects whose introspections have inherent value. Thus, the Bühler investigation of thought, whatever criticism be made on its method, will ever retain significance precisely because Külpe and Dürr were its subjects. And, in the second place, this form of systematic introspection, though it admits no repetition and variation of stimuli, yet provides for the use of questions conforming to different marked types of interest and for a repetition not indeed of the questions themselves but of the types. Wundt ignores this feature of the question-method and his attack on Bühler seems, in so far, indiscriminating and unjustified.

In spite of these acknowledgments it must, however, be admitted that non-experimental introspection suffers from lack of objective control and thus from absolute dependence on the unchecked introspection of the observer, and from the constant temptation to attack large problems, insufficiently

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<sup>32</sup> Cf. pp. 505ff. below.

<sup>33</sup> Cf. pp. 508ff. below.

<sup>34</sup> Cf. pp. 511ff. below.

analyzed, by methods too untechnical.<sup>35</sup> At all these points the advantage lies with experimental introspection which unquestionably more closely approaches the ideal of the scientist. For by this procedure, as has appeared, it is possible to repeat accurately, to vary widely and often exactly, and to compare (often by measurement) not only the concrete physical conditions or part-conditions but sometimes also certain physiological conditions of the perception or imagination, the emotion, the recognition, or the activity under investigation. This fundamental superiority is supplemented by two other advantages. In the first place, the objective results—numbers of color mixtures rightly made, of syllables correctly remembered, or of choices of one or another type—these numerical results may have value in themselves and may even be of indirect use in the analysis of the introspections. Second, and perhaps more important, the experimental environment and procedure in itself disposes the trained subject to the even, alert, unemotional attitude of the good introspector.

The method of experimental introspection as applied to experiences whose physical conditions are very incompletely controlled, is still in its infancy. In the nature of the case it can proceed but slowly because of the unprecedented demands which it makes. Foremost of these, it has already appeared, is the imperative requirement of thoroughly trained subjects. When, as in the simpler forms of psychological experiment—in sensation, reaction, and memory-experiments—objective results are important, these can evidently be obtained from subjects with relatively little training, provided they are numerous enough and closely superintended. When, however, the results of introspection constitute not merely an integral factor but the special object of study, it is necessary to have subjects at once highly-trained and unprejudiced. Almost equally necessary is an investigator who is himself well-trained in introspection and gifted in the understanding of introspecters' reports. Mechanical skill in the manipulation of apparatus, accuracy in reporting, industry in the calculation of medians, averages, and mean variations, can never take the place of these essential requisites.

Considering the difficulties involved, the progress of the method of experimental introspection, thus applied, has been not inconsiderable. By means of it scientific psychology is slowly advancing its boundaries beyond the confines of sense-psychology. In particular, judgment and comparison, recog-

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<sup>35</sup> Cf. Titchener, "Experimental Psychology of the Thought Processes," pp. 96f.



nition, and volition have been investigated by methods of observation more or less strictly regulated. In controlled introspection, thus conceived, the self if it exists should be found. Critics of self-psychology are amply justified in insisting that the self can have no status in scientific psychology unless it be discovered by this method. In the following sections of this paper the attempt will be made to show that experimentally controlled introspections have discovered the self and to explain why the "returns" are relatively so scanty.

## B. THE SELF AS DISCLOSED BY INTROSPECTION

### I. *The Self in Experimental Introspection*

This section will summarize the results of certain experimental introspections which disclose a self. The earliest record known to the writer, in the literature of strictly experimental introspection, of an explicit consciousness of self is made by E. Dürr. His subjects, he says, "have often reported: This . . . experience appears to us an especially striking act of will, for we felt ourselves concerned in it (*dabei engagiert*) with our whole personality." "It is a fact," Dürr says, a little later, "that the subject experiences a vivid, continuous consciousness of self."<sup>36</sup> In Dürr's study, however, the consideration of self is purely incidental. The following pages will summarize detailed introspections in which the self plays an important part.

#### a. *Katzaroff's Study of Recognition*<sup>37</sup>

Katzaroff exhibited to his subjects geometrical drawings each occupying 2 to 4 square cms. on a white background of 6 x 9 cm. The drawings were grouped in series of six and three series were presented to each subject at an experimental sitting. The drawings on which Katzaroff's paper is based were fixated each for six seconds; and each was shown after an interval of four seconds from the disappearance of the next preceding. A five-minute interval separated the series. Five minutes after the appearance of the last series, the test-series was shown. This was made up of the 18 'normal' drawings already shown (N) mixed with 18 similar but 'varying' drawings (V). The subjects were ten in num-

<sup>36</sup> E. Dürr, "Die Lehre von der Aufmerksamkeit," 1907, pp. 73<sup>1</sup>, 74<sup>2</sup>, 75<sup>2</sup>.

<sup>37</sup> D. Katzaroff, "Contribution a l'Étude de la Récognition," *Archives de Psychologie*, 1911, Vol. XI., pp. 1-78. The parenthesized references of this section are to this monograph.

ber, five men and five women, nine of them students and one a professor. They knew that the test series were made up partly of repeated and partly of new diagrams. Every subject took as long a time as he wished to respond 'yes' or 'no'—meaning 'familiar' and 'unfamiliar'—as each drawing was shown; and he was then asked to "describe as completely as possible his whole experience (*tout ce qui s' était passé en lui*) from the moment of the appearance of the drawing up to the time of his reply."<sup>38</sup>

Katzaroff's study is based on 1,100 such records. Only one of his subjects, presumably the professor, knew the purpose of the study; the others supposed that he was investigating reproductive memory. Both the 'objective' and the 'subjective' results of the investigation are summarized. Under the first head are tabulated the relations of true and false recognition. (Katzaroff applies the term 'true recognition' as well to cases in which the novel figure is called 'unfamiliar' as to cases in which the repeated figure is called 'familiar.') He makes the following points among others: (1) that the correct recognitions are in the majority (pp. 30-31); (2) that four degrees of certitude may be distinguished (p. 32); (3) that the time taken for recognition reveals the quality rather than the correctness of recognition (p. 37). By this Katzaroff means that when associated images "play the chief rôle in recognition" the time required for designating novel designs as unfamiliar is less than that for designating repeated images as familiar, whereas the opposite holds when the preponderant rôle in recognition belongs to the feeling of familiarity (*le sentiment de connu et de familier*).

But this summary of Katzaroff's conclusions about numbers and times of correct and false recognitions is merely preliminary to the study of what he calls his subjective results. The following are illustrative extracts from his subjects' records:

"Ex. 18 (N): + Seen before; very sure; I remember that I attended to the shape of the cube and to the apices and the sides of the triangle. I said to myself that I could easily reproduce them (p. 46)."

"Ex. 51, fig. 6, (V):—Seen before, very sure. As always, I immediately experienced the feeling of familiar (*le sentiment de connu*). Then I remembered that I had examined the diagram several times since fixating it and that I had taken sharp notice of the little circle and of the vertical line in the little indentation of the circumference.—(N): + Seen be-

<sup>38</sup> For the description of procedure cf. Katzaroff, *op. cit.* pp. 29f.

fore, very certain; even while I was writing the above I felt a little doubt about the details but the design seemed so familiar to me that I did not pay attention especially since they were the same. But now I recognize well the position of the details (p. 62)."

It is perhaps unnecessary to point out that these are unsatisfactory introspective records, in themselves indicative of little training on the part of most of Katzaroff's subjects. The records abound in the unanalytic terms of uncritical observers, as when, for example, Katzaroff's introspecters talk of attending to 'cubes' and 'perpendiculars,' instead of analyzing out of these experiences the kinaesthetic and visual sensations which they contain. Opinions will differ as regards the degree to which the inadequacy of the records affects the correctness of the conclusions.

These conclusions may be summarized as follows: (1) Associated images (though essential to indirect and to complete recognition and though often serving to confirm the certainty that a recognition is correct) neither *constitute* nor *determine* recognition. Almost always, indeed, the images associated by the recognized drawing follow upon the recognition or consciousness of familiarity (pp. 46-48).

(2) Recognizing, in the second place, can not consist in comparing the percept of a given object with the memory-image of the same object, for Katzaroff's subjects never base their recognition on the completeness of a memory-image and, indeed, their memory-images never are complete and often are inaccurate (p. 77<sup>2</sup>).

(3) The essential factor in recognition, according to the introspective records, is the feeling of familiarity—what Katzaroff calls '*l'impression ou [le] sentiment de familier*' (p. 49<sup>3</sup>),<sup>39</sup> '*le sentiment de familier, de connu, de déjà vu*' (p. 44<sup>1</sup>). This feeling he describes as immediate (p. 44<sup>3</sup>), as 'accompanied often by a feeling of satisfaction' (p. 44)<sup>40</sup> and more fully as "a feeling . . . that a thing already belongs to one's experience (p. 77<sup>1</sup>). . . . The feeling of the familiar, the 'seen before,'" he continues, "which accompanies a repeated sensation arises from the fact that this very sensation has connected itself with the very feeling of our 'self' and has been enveloped by this feeling of self (*s'est associée au sentiment lui-même de notre 'moi,' a été envelopée par celui ci.*" (p. 78<sup>2</sup>. Italics Katzaroff's.)

<sup>39</sup> Cf. pp. 50, 75.

<sup>40</sup> He sometimes designates this as the 'affective element (*l'élément affectif*)' Cf. pp. 49, 53.

Evidently Katzaroff finds clear evidence of a self in recognition.

*b. The Study of Michotte and Prüm on 'Voluntary Choice'*<sup>41</sup>

The method and the procedure of the experiments of Michotte and Prüm may be summarized as follows (pp. 128 ff.):—The subject, always alone in a room, was told that a card containing two numerals would be shown him. He then chose between two arithmetical operations, for instance, between multiplying and dividing the numerals in question. The choice was to be made "for serious considerations." (This direction was given "to preclude mechanical reactions (cf. pp. 140-141, 159ff).") Then, without carrying through the operation (pp. 129ff), the subject reacted by touching a key. This concluded Period 1 of the experiment. In Period 2, the subject, still entirely alone, "devoted himself as painstakingly as possible to introspection, reviewing everything from the moment of the appearance of the stimulus. As soon as this observation was concluded and the different states fixed in his memory he gave a signal to the experimenter waiting in the next room" and then dictated to him the results of the introspection (p. 143<sup>2</sup>). The subjects were Michotte (in 220 observations), Prüm (in 122) and four other subjects in 95, 90, 48 and 20 experiments respectively. All agreed that they made genuine choices, resembling ordinary voluntary decisions (p. 130).

Introspective records and numerical results are given in great detail and with ample comment. Among the more important topics which are discussed are the perception of the stimulus, the nature of alternatives, and the effect of instruction. Particularly noteworthy chapters describe the 'intermediary' and the 'final' stages of choice. The factors of the former are the consciousness of doubt and of expectation closely fused with strong muscular sensations. In the final stage "expectation is fulfilled, doubt gives place to certainty and, in general, muscular tension disappears" (p. 182<sup>3</sup>).

This final stage of choice may, and often does, include (1) verbal image, (2) kinaesthetic consciousness due to head movements and to intake of the breath and (3) affective experience; but its only essential character (pp. 194<sup>2</sup>-195<sup>1</sup>) is that which the authors call the 'consciousness of activity'

<sup>41</sup> "Étude Expérimentale sur le Choix Volontaire et Ses Antécédents Immédiats," par MM. A. Michotte et E. Prüm. *Archives de Psychologie*, 1911, X., pp. 113-320. The parenthesized references in the following pages are to this monograph.

—an experience, as they expressly state, which is no “distinct content by the side of others, and is absolutely different from the feeling of muscular activity (p. 194).” On the positive side, they characterize it as “leading directly to the affirmation of the intervention of the self” and as describable only through the use of infinitives, such as ‘faire,’ ‘agir,’ ‘se tourner vers,’ ‘laisser aller’ (pp. 193-194<sup>1</sup>).

The following are representative introspective records:—  
M. 185: “On hearing the directions read I felt a strong muscular contraction in my chest with a feeling of agitation and unpleasantness. Then I said to myself: ‘Nevertheless I ought to do it’ (p. 154).”

P. 512—“In acoustic-motor form, ‘division!’ The articulatory sensations were rather strong in the moment of pronouncing; at the same time a slight inclination of the head and an ‘awareness’ (*savoir*) that it was *I* who acted (*agissais*) . . . (p. 189).”

M. 419. “At the decision the consciousness of determination was very definite. I notice this particularly when I compare this phenomenon with the manner in which a content of consciousness appears through association. In the latter case I cannot say, as here, ‘I say it.’ On the contrary, as associated, the content is pronounced of itself. Herein, in particular, I find what I call ‘willed,’ voluntary. It is something absolutely different from the feeling of muscular activity” (p. 192).

M. 433. “The feeling of expectation was suddenly interrupted by decision. The consciousness of acting, of doing, of determining, was clearly presented, though there was no representation of the *I* and though the *I* was present in no other explicit way. At the same time I felt this muscular activity, a muscular contraction, but the consciousness of activity was totally different from this and from the feeling of relaxing tension.” (P. 192.)

These records confirm the insistent assertions of the authors that the consciousness of activity, so prominent in their recorded choices, is not to be identified with kinaesthetic sensations, with verbal images, or with affective consciousness. For these introspecters report their sensational and affective experiences but explicitly distinguish them from the consciousness which they describe in such phrases as: “it was *I* who acted.” In the authors’ view, the activity consciousness is indefinable: it is an ultimate datum (*une donnée dernière*, p. 311). It so easily escapes detection, simply because other more vivid factors of the total willing consciousness “impose themselves on it (p. 312<sup>2</sup>).” And, whether

explicit or implicit, "the I manifests itself in a wholly special way in the act of will (p. 311<sup>8</sup>)."

Of great interest is the more detailed analysis of the activity consciousness. The authors find activity present not only in will but in tendency, desire, and wish (p. 195<sup>3</sup>) and even in submission, or consciousness of passivity, which they sharply distinguish from the inactive, or receptive, consciousness.<sup>42</sup> They enlarge the meaning of the words 'choice' and 'decision' using these terms to cover involuntary, automatic, as well as truly voluntary responses (pp. 204ff). Under 'voluntary choices,' they distinguish true decisions from consents in which the chosen alternative alone is present or strongly favored (pp. 190 *et al*).<sup>43</sup>

An important conclusion which Michotte and Prüm base on the recorded introspections has still to be stated. After-reflection on the consciousness of activity, leads their introspecters, they tell us, to the conclusion that the self, in choice, is causal; and this conceptual consciousness of the causal self appears not in the immediate experience of choice but in the reflection upon it.<sup>44</sup> This teaching, that "the concept of the I is not found in the moment of voluntary action but that it imposes itself on the subject when he is set to describing exactly what has passed" (p. 192) is not unlikely to be taken by the critic of self-psychology, as evidence that the consciousness of self reported by these observers is a philosophical afterthought, an intrusion due to the 'psychologist's fallacy.' Certain statements of Michotte and Prüm lend themselves to this interpretation. Yet it is difficult to believe that they hold this view and wholly impossible to reach this conclusion from a study of their recorded introspections. For these justify not the denial of an immediate awareness of the self as choosing but merely the denial of an immediate consciousness (or an explicit consciousness) of the self as causal in willing. Such an awareness of the causal self is, however, admitted by everybody to be a conceptual, philosophical experience.

It may be added that the past tense in which the consciousness of self is recorded—for example, in the statement "*c'était moi qui agissais*"—is inevitable under the conditions of the experiment which required that report be a repetition of silent

<sup>42</sup> Cf. the writer's "A First Book in Psychology," pp. 244ff, 252ff., for the same view (except as regards the use of the term 'passivity').

<sup>43</sup> Cf. William James's distinction of choices with and without effort.

<sup>44</sup> On the appearance, after successful volition, of the consciousness of the self as cause, cf. Ach, *op. cit. infra*, pp. 265ff.

introspection. In truth, this past tense is used not only of the consciousness of self but, of series of images in such statements as, "il y avait une image visuelle."

The fact that Michotte and Prüm regard their results as essentially agreeing with Ach's clearly confirms this interpretation of their conception of will as self-activity. For, as will appear immediately, Ach unambiguously finds in willing an activity of the self.

### *c. Ach's Second Experimental Study of Volition<sup>45</sup>*

The method employed by Ach in this investigation was the following: Using an apparatus similar to that of Müller and Pilzecker (p. 24), and with the usual precautions against time and space-errors (pp. 29ff) he exposed before his subjects 8-syllable series of nonsense-syllables to be memorized. One of these series, the normal (gewöhnliche or *g*) series, was made up, like the ordinary memory-series, of unrelated syllables (p. 20); a second series (*r*) was composed of rhymed syllables, *zup tup, mär pär, bis zis, tel mel* (p. 26); in a third (the *umgestellte* or *u* series) the consonants of successive syllables were reversed; *dus sud, rol lor, nef fen, mön nöm* (p. 25<sup>4</sup>). In some of the experiments the reversed series were omitted (pp. 56ff); and in certain groups of experiments word-syllables replaced the nonsense-syllables as in *Staf-fel, Rit-ter, Gar-ten, Pfir-sich, Nürn-berg* (pp. 182ff).

After the series had been repeated a given number of times, so as to form associations with each other, pair by pair, the odd-numbered syllables were repeated and the subject was either directed to respond to each, as he heard it, with the originally associated syllable or else he was told to respond by a syllable of another indicated type. In Procedure I, for example, which was designed to facilitate strong associations between syllables, the single series of normal (*g*), reversed (*u*), and rhymed (*r*) series were read 20 times each on the first day (each series 8 times, followed by each series 6 times, and again by each series 6 times—60 series in all, p. 28<sup>4</sup>). On the six following days each series was read 10 times (p. 29<sup>1</sup>). With the seventh day began the test-series. In these the 12 odd-numbered syllables (the first, third, fifth, and seventh of each of the series) were supplemented by 4 new syllables (*n*), shuffled,<sup>46</sup> and shown the subject ten times

<sup>45</sup> N. Ach "Über den Willensakt und das Temperament," Leipzig, 1910. Parenthesized references in the pages which follow are to this work.

<sup>46</sup> For the exact arrangement, the text of Ach should be consulted.

each; and he was now required to react in a novel fashion—for example, to rhyme a syllable which in the series, so often repeated, had been followed by a 'normal' one or to 'reverse' a syllable previously rhymed. The specific purpose of the experiment was to discover the effectiveness of the will-to-respond by a new syllable of definite type, as measured against the habit-of-responding with the syllable learned through repetition of the series. The number of repetitions which had to be just exceeded in order that the reaction should be due not to the will to respond according to instruction but to the associations already formed is called "the associative equivalent of the will."<sup>47</sup> For one of his subjects, A, Ach finds, for example, that given a moderate concentration of the will, when (a) the series of reversed syllables has been learned by the many repetitions of Procedure I. and when (b) the requirement is to react to the syllables, displayed in the test series, by rhymed syllables in place of the former 'reversed' associates, "the associative equivalent of will lies between 100 and 120 repetitions" (p. 45<sup>8</sup>). This means that, if the reversed series (dus sud, rol lor, nef fen, mön nöm) had been 100 to 120 times repeated, subject A was just unable to respond to 'nef' by a rhymed syllable. Whereas, if the learned series had been only 90 times repeated he could, ordinarily at least, carry out his will to rhyme the syllable.

The two first chapters of Ach's book contain the further details of his procedure and discuss in detail the quantitative, or objective, results of his experiments. By varying the character of his series, and the number of repetitions, he was able to vary the strength of the associations between successive syllables, and thus to obtain an objective estimate of the concentration of the will required to overcome the associative obstacles. The effect of these varying obstacles was manifested in the lengthening of the willed reaction, in the occurrence of false reactions, and in paraphrased or hesitating reactions.

It would, however, be beside our purpose to discuss at length these 'objective' results. For our main concern is with the introspections reported by the subjects of these experiments and with the analysis of will based by Ach upon these introspections. It is to be regretted that Ach sum-

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<sup>47</sup> "Diejenige Zahl von Wiederholungen einer Silbenreihe welche eben überschritten werden muss damit die gestiftete Assoziation und nicht die Determination den Ablauf des Geschehens bestimmt, bezeichnen wir, als das assoziative Äquivalent der Determination," p. 43<sup>3</sup>.



marizes and only briefly quotes from these introspections instead of transcribing his introspective protocols in full. We are none the less justified in attributing a genuine value to Ach's records and to the conclusions which he bases on them. For the abbreviated method of presenting introspective reports is identical with that of Ach's earlier work, "Über die Willenstätigkeit und das Denken;" and competent experimentalists have commended the method of this earlier investigation and accepted, with greater or less modification, its conception of determining tendencies or dispositions.

The following are representative passages of Ach's records of introspections:

Subject R. Procedure IV. (Word-syllables.) Instruction: to rhyme. Following upon a false reaction "an energetic act of will began in the introductory period of the next experiment. Strong fixation of the field with the expectation that one of the accustomed syllables would appear there. Hereupon, immediate transition to the resolve 'I will this time really pronounce a rhyme' (an awareness in which 'I can' as well as 'I will' was included). Then the perception of the field vanished and very vivid sensations of strain were experienced in the forehead and in the organs of speech." (Pp. 231-232.)

Subject R. Procedure IV. Introductory Period. Instruction: to rhyme. "Fixation of the screen with weak strain-sensations in the forehead, then [the word] 'Stamm' visually projected with the consciousness 'I will form a rhyme to a word of that type.' Then inner speech 'I will' with the meaning 'I will form a correct rhyme' (a sudden (*stossartiger*) energetic resolve with intensive concentration, greater predominance of the strain sensations in the organs of speech and disappearance of the visual image *Stamm*)."

Subject K. Procedure IV. "Instruction: 'Energetically resolve immediately after the reading to form a rhyme.' Hereupon resolve with the consciousness 'I will form a rhyme with the [word] which is going to appear at the point which I am fixating,' with strong strain-sensations in the abdomen, larynx, and forehead. (These sensations were stronger than in the preceding experiments.)" (P. 192.)

On the basis of many such introspections Ach analyzes the primary or 'energetic' volition into four phases or 'moments' (p. 247) ultimately fused: First, the perceptual phase (*das anschauliche Moment*) made up of the strain-sensations which characterize attention (p. 238). Second, the objective phase,

the consciousness—usually imagined in verbal terms but sometimes an imageless awareness, or *Bewusstheit* (pp. 240, 138)—of the end or aim of will (pp. 239f). Third, and most important, the actual phase, described by Ach as an activity (*Betätigung*), an attitude (*Stellungnahme*), the consciousness, “I will.” (Pp. 240 *et al.*, 292<sup>2</sup>, 242<sup>3</sup>.) Fourth and finally, the consciousness of difficulty and of exertion (*Anstrengung*, p. 245). Ach contrasts this as a state (*zuständliches Moment*) from the activity of will, and groups it—along with doubt, perplexity, and wonder—among Marbe’s *Bewusstseinslagen* (pp. 9f.).

Of these four ‘phases,’ the consciousness of end and the self-activity are most important and are discussed in close relation to each other. The end of will is, in Ach’s experiments, always itself an action—to make a rhyme, or to reverse consonants; and this action, furthermore, is not ‘action in general’ but always a deed which *I* am to perform (p. 240<sup>2</sup>), that is, it includes the consciousness of self. But this specific act, which may constitute the end and outcome of will, must be most carefully distinguished from the activity (*Betätigung*) in which will consists (p. 240<sup>2</sup> *et al.*) and in which, Ach reiterates, the self-aspect (*die Ichseite*) of psychic events is peculiarly prominent (p. 241<sup>2</sup>).<sup>48</sup> The *I* constitutes, in truth, the starting-point of the relation to the objective moment, that is, to the end (p. 244<sup>5</sup>). Such a consciousness of the end to be realized through the self brings with it the consciousness of “the exclusion of every other possibility” (pp. 241-242), and in Ach’s view makes explainable the actual, objective effectiveness which distinguishes will from mere intention (pp. 248-249).<sup>49</sup>

Ach attributes one further character to primary, or energetic, willing. At this level, he teaches, self-activity is always fused with a consciousness of future achievement; and “ich

<sup>48</sup> Meumann, “Intelligenz und Wille,” 2te Aufl., p. 229, is at pains to point out that this conception of the will as self-activity contains nothing essentially new as compared with Meumann’s theory, set forth in the first edition of “Intelligenz und Wille.” This is true; and a similar comment might be made on Meumann’s original statement.

<sup>49</sup> This teaching of the effectiveness of will and the conception of the objective phase of volition constitute the main points of contact between Ach’s earlier and later doctrine of volition. The later teaching is in no sense opposed to the earlier, though it is stated in relative independence of it. The ‘determining tendencies,’ though not infrequently referred to, more often retire to the background. (Cf., however, pp. 249-256, the section on “Die dynamische Seite—Wirkungsgrad des Wollens.”)

will" is equivalent to "ich will wirklich." In other words, the I realizes itself as antecedent of the end to be achieved and conversely realizes the object of will as dependent on itself.<sup>50</sup>

In the succeeding sections of chapter III. Ach discusses attainment of purpose and failure to attain, with their results. The most significant of his teachings concerns the experience of power or ability. The consciousness of achievement which follows on the attainment of the willed purpose includes, he says, the consciousness expressed in the words 'I can.' This is the kernel of the feeling of independence and of the emotion of self-respect; it is the basis of my belief in my freedom of choice. (P. 268; cf. p. 244<sup>3</sup>.)

Ach's analysis of volition and his discussion of achievement are followed by a consideration of three sub-forms of volition: the abbreviated, the weak, and the habitual volition. Most important is the weak volition (*das schwache Wollen*) which is differentiated by the fact that the emphasized consciousness "I will," essential to primary volition, is replaced by a two-fold consciousness, the relatively impersonal consciousness, "this is to happen (*es soll*)" and the relatively inactive consciousness, "I am ready (*Ich bin bereit*)." Obviously the exclusion of other possibilities and the anticipated realization or bringing-to-pass of the end are lacking to volition in this weakened form (pp. 280ff). An allied form of weakened will is the experience expressed in the words "I must" or 'I ought'—an experience which implies unconditioned subordination of oneself to the task assigned (the *Aufgabe*). (P. 292<sup>2</sup>. Cf. p. 244<sup>2</sup>.)

Ach's discussion, in truth, fairly bristles with topics of interest to the investigator of volition. Such, for example, are his assertion that his observers never named pleasure (*Lust*) or its opposite (*Unlust*) in describing the 'primary act of will' (p. 246<sup>3</sup>), and his insistence that not the instruction, or *Aufgabe*, as such, but the undertaking or receiving of instruction, is to be looked on as a determining factor of choice (p. 284).

But we are reading Ach for an answer to the question: does experimental introspection disclose the self? The answer is unequivocally affirmative: By his phrase 'the self-aspect

<sup>50</sup> The observers in the investigation of Michotte and Prüm (pp. 508ff. above) do not report this consciousness of future realization and the authors, rightly in my opinion, point out that such an awareness is not a necessary character of will, however invariably present in volitions like those of Ach's subjects. Cf. Michotte and Prüm, *op. cit.*, pp. 314<sup>3</sup>ff.

of psychic phenomena' he implies that all experience involves consciousness of self. And he unequivocally asserts the rôle of the self in volition; first, in the consciousness of the end, or aim, as that which I am to carry through in the future; second, in the consciousness of preparedness, the "I am ready to accomplish this when it is due" which distinguishes the weakened will; and pre-eminently in the complete self-activity of primary will. Finally, Ach offers us at least a promising beginning of a scientific description of the willing self. He rightly stresses self-activity as the elemental aspect which distinguishes will, though he finds in will not only this consciousness of activity but other characters as well.<sup>51</sup> And he classifies the forms of will according to the relative predominance of the activity-consciousness.<sup>52</sup>

Especially to be stressed, in conclusion, is the fact that Ach definitely asserts that the self is experienced (*erlebt*) and not merely inferred to exist.

## II. *The Self in Non-experimental Introspection*

The preceding section summarizes the work of investigators who assert the existence of self on the ground of immediate experimental introspection. The list of psychologists who, more or less emphatically acknowledge a self is, however, barely begun. The present section names some of these writers who, presumably, base their statements either on incidental or systematic observation even when they do not explicitly say so. One group of writers is, so far as possible, excluded, the adherents of the merely-inferred-self hypothesis. These writers agree with the believers in selfless psychology that consciousness can be adequately described without having recourse to a self; though they also hold that certain characters of consciousness presuppose the existence of a self fundamental to specific experiences. With such an inferred self, however, the philosopher, not the psychologist is con-

<sup>51</sup> The passage (end of page 240 and first part of page 241) in which Ach describes activity as elemental is not free from ambiguity. I interpret him as teaching, correctly enough, that activity is a  *sui generis*  phase or attitude of the self in relation to its object. Meumann's claim (*op. cit.*, 2nd edition, p. 230<sup>2</sup>) that activity is analyzable seems to rest on an identification of activity with will.

<sup>52</sup> A suggestion of the further classification into self-assertive and adoptive will—the active and passive will of Michotte and Prüm (p. 510 above)—may be found in the discussion of the consciousnesses 'I must' and 'I ought.' Cf. the writer's Introduction to Psychology (1901) pp. 306ff. and "A First Book in Psychology," 4th edition, pp. 244ff.

cerned, for the psychologist studies an experience as immediately realized not as later reflected on. To infer, for example, that a self must have been present in a previous recognition does not prove that the recognition was an experience of this self; yet only a self thus directly experienced can be dealt with in the psychology of recognition. In truth the self, thus conceived, plays a rôle in psychology somewhat comparable to that of matter or of the molecule in physics—the rôle not of an observed fact but of an inferred, and thus hypothetical, condition of observed facts.<sup>53</sup>

Self-psychologists, excluding thus the purely philosophical, are of two groups. (1) The first and largest group includes those who hold that we are conscious of self in certain experiences and not in others. In particular, many psychologists, of most divergent views, describe—or have described—the affective consciousness as 'subjective.'<sup>54</sup> Recognition, as well as emotion, is found by a growing number of psychologists to involve the consciousness of self. Katzaroff<sup>55</sup> includes James Mill,<sup>56</sup> J. M. Baldwin,<sup>57</sup> William James,<sup>58</sup> Edouard Claparède<sup>59</sup> and Netschajeff<sup>60</sup> among the upholders of this view that recognition consists in the consciousness of myself experiencing what I have previously experienced. In addition E. Dürr should be named, for he says explicitly "It is an act of self-consciousness which . . . constitutes (*ausmacht*) memory."<sup>61</sup> In will and belief and

<sup>53</sup> In the opinion of the writer an immediate consciousness of self is, none the less, involved in the philosophically-minded psychologist's *present* experience of inferring that there was a self. This, however, is not the time at which to argue this question or to enter on a discussion of the nature of inference.

<sup>54</sup> The conception of the affective consciousness as subjective is so well known that it is unnecessary to give references. It should be added, however, that many writers who find the self in feeling use the term 'feeling (*Gefühl*)' in a very wide sense to include will, striving and (in general) the non-perceptual consciousness. This is probably the usage of Lipps and is almost certainly the meaning of Oesterreich (*op cit.*, pp. 13, 230) and Pfänder (*Phänomenologie des Wollens*, 64ff.).

<sup>55</sup> *Op. cit.*, pp. 23ff.

<sup>56</sup> *Op. cit.*, Chap. X.

<sup>57</sup> *Handbook of Psychology*, p. 178.

<sup>58</sup> *Psychology*, I., p. 650.

<sup>59</sup> Recognition et Moitié, *Archives de Psychologie*, 1911, XI., pp. 79ff.; "Exp. sur la mémoire dans un cas de Psychose de Karsakoff," *Revue med. de Suisse romande*, 1907, p. 301, cited by Katzaroff.

<sup>60</sup> "Association par ressemblance," 1905, cited by Katzaroff, *op. cit.*, p. 25.

<sup>61</sup> "Grundzüge der Psychologie" von H. Ebbinghaus, fortgeführt von E. Dürr, Bd. II., S. 243. Dürr, like Mill, uses 'memory' in the sense of 'recognition.'

kindred experiences, other writers find the direct consciousness of active, of submissive, or of adoptive self. Meumann, for example, says explicitly that will consists in "my knowing (1) that I am the one assenting to the purpose (*der dem Ziele zustimmende*) and (2) that this assent and my own fixation of the purpose is what controls the psychic mechanism of the execution of the activity (*Handlung*)."<sup>62</sup>

C. S. Myers, finally, attributes self-consciousness to all psychic experiences except the purely sensational. "We experience," he says, "'acts' of apperception, thinking, willing, imagining, etc., in all of which the self is involved."<sup>63</sup>

(2) Such a view as that of Professor Myers shades almost imperceptibly into the conception of self-psychology in the fullest sense of the term—the view that all consciousness is a consciousness of self and that the psychologist, therefore, willy-nilly studies the self. According to this conception, the self as object of direct psychological study is roughly comparable not with ether or with molecule but—let us say—with light or with air.

This paper is concerned with self-psychology of every sort and not with the matters on which self-psychologists divide. Yet it may be proper, at this point, to remark on one advantage in the complete form of self-psychology. This advantage is brought out by the examination of a great difficulty with the other theory—the view, namely, that we are sometimes but not always conscious of self, in particular, that we are aware of self in feeling and in willing only, not in perceiving and imagining. For, if we can not feel, will, or doubt without consciousness of self, it seems antecedently probable that the self is essential also to perception. A similar difficulty is involved in the kindred view, that the mature and civilized man, not the child or the savage or the animal, has the consciousness of self. For here again there emerges the problem of explaining why and at what point self-consciousness should be superimposed upon consciousness. Once grant the occurrence of consciousness of self and it is at least simpler to

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<sup>62</sup> "Intelligenz und Wille," 2nd edition, 1913, p. 238. Cf. 1st edition, pp. 289-290. Cf. Shand, "Types of Will," p. 300, cited by Michotte and Prüm.

<sup>63</sup> *British Journal of Psychology*, 1913, VI., p. 153. Cf. p. 140; where Myers argues that "we should make two main divisions of consciousness—the consciousness of 'acts' or 'processes' (*e. g.*, the 'acts' of attending, imagining, . . . , thinking, willing) and the consciousness of 'contents' or 'products' (*e. g.*, 'what' we attend to, 'what' we imagine . . . )."

suppose it always present.<sup>64</sup> The unwillingness to hold that the self is invariably experienced in every consciousness is, in truth, due to the mistaken view that it is to be found only in complex and predominantly intellectual experiences, whereas it may as truly be *felt* in every primitive and inchoate consciousness.

Self-psychology in its completed form, the doctrine that consciousness involves always a consciousness of self has been explicitly avowed by James Ward,<sup>65</sup> by J. Rehmke,<sup>66</sup> by Robert M. Yerkes,<sup>67</sup> by Eleanor A. McC. Gamble,<sup>68</sup> by W. Mitchell,<sup>68</sup> and by the writer. James R. Angell and C. S. Judd should probably be added to this list. Angell speaks of 'the concrete actual self' and says: "the normal human mind is never a mere string of states of consciousness. It is always a unitary affair in which the past, the present and even the future are felt to hang together in an intimate personal way."<sup>69</sup> And Judd mentions without disapproval the view that "every human being has a direct recognition of the self from the first," provided this doctrine be supplemented by the obvious qualification that the degree of this immediate consciousness of self is not the same in early childhood and in mature life. Finally, E. Meumann,<sup>70</sup> E. Dürr,<sup>71</sup> A. Pfänder,<sup>72</sup> and N. Ach<sup>73</sup> should be included among those who implicitly conceive of experience as always involving the consciousness of self.

### C. EXPLANATION OF THE FAILURE TO DISCOVER SELF BY INTROSPECTION

The foregoing pages have amply shown that many psychologists believe themselves to be conscious of self; and that

<sup>64</sup> This resembles that argument, to show that consciousness is co-extensive with life, which is based on the irrationality of the supposition that consciousness suddenly appeared at any one stage of development. Cf. G. H. Schneider, "Der thierische Wille," Kap. V., Binet "The Psychic Life of Micro-organisms," E. B. Titchener, *Popular Science Monthly*, 1902, Vol. LX., pp. 465ff., Wundt, *Grundriss*, IV., sec. 19.

<sup>65</sup> Cf. *Encycl. Britannica*, 9th and 11th editions, article on Psychology, and "The Definition of Psychology," quoted p. 497 above.

<sup>66</sup> *Op. cit.*

<sup>67</sup> Cf. his "Introduction to Psychology," pp. 15, 17, 53 *et passim*.

<sup>68</sup> *Op. cit.*

<sup>69</sup> Psychology, ch. XXIII., 4th ed., p. 440.

<sup>70</sup> *Op. cit.*, pp. 350, 351.

<sup>71</sup> "Grundzüge der Psychologie," II., H. Ebbinghaus, fortgeführt von E. Dürr, p. 230.

<sup>72</sup> "Einführung in die Psychologie," pp. 374f., 380ff., and *Die Phänomenologie des Wollens*, p. 14<sup>3</sup>.

<sup>73</sup> Cf. above, p. 514. Cf. P. Natorp, "Allgemeine Psychologie," pp. 23, 29.

not only casual but experimental introspections have discovered this self. But an insistent question remains: The self-psychologist has still to explain the fact that a large number, perhaps a majority, of psychologists deny or ignore the self. In a general way this attitude of hostility or of unconcern is, of course, accounted for by the fact that the psychologists in question seem to themselves to find no traces of self in their own introspections and in those reported by their subjects and that they consequently question the accuracy of reported introspections of a self. The problem which confronts the self-psychologist may therefore be stated in the following terms: If a self can really be found by scientific introspection why is not the consciousness of self reported by every skilled introspector?

In his attempt to answer this question the self-psychologist will first of all take issue with the statement of the facts. He finds that the technical writings of selfless psychologists are full of the terms of self-psychology; that they bristle with references to experiences, of which 'I take note,' or in which 'I am concerned,' or with definitions of psychical reality as 'that which is experienced by a single subject.' In a word, selfless psychologists are constantly implying a subject, or self, and are perpetually distinguishing it from its experiences. To the self-psychologist it seems futile to explain this inveterate habit of referring to the self by the convention of language, for the convention of acknowledging a self is precisely the fact to be explained.

Even in experimental protocols selfless psychologists talk of 'subjective' attitudes and distinguish between 'subjective' and 'objective' introspectors. And in the hard-worked contemporary concepts of 'attitude,' (*Bewusstseinslage*), and 'instruction,' or 'problem' (*Aufgabe*), as well as in the resuscitated categories of 'activity' and 'disposition,' the self-psychologist finds the most evident implications of self. It seems to him impossible that a consciousness of instruction should occur except in the form of an awareness of oneself being instructed by some one; and equally impossible that one should be conscious of attitude, of activity, or of disposition except as one is aware of a self which is active, has dispositions, and takes attitudes.

Yet, though he heaps up the circumstantial evidence that selfless psychologists are forever presupposing the self, the self-psychologist finds it very difficult, if not impossible, in any specific case to disprove the selfless psychologist's assertion that the terms 'I' and 'you' are mere 'verbal labels.'



For this reason, and because the underlying purpose of this paper is to make clear the point of view of self-psychology, rather than to confute its critics, it will be assumed in the remainder of this discussion that the facts are as stated by the opponents of self-psychology; that is, that a large number of trained observers fail to find the self in introspection. The self-psychologist can, however, offer four reasons to account for this failure. These are (1) the ubiquity of the self; (2) the fact that systematic introspection has concerned itself mainly with sensational experiences and with thought; (3) the character of the specific directions often given to introspecters; and (4) the fact that conceptions of introspection and of report often virtually or explicitly exclude all reference to the self.

(1) The fact, so often already emphasized, that I am always conscious of the self tends directly to make me inattentive to it. Just as, if I were asked to report fully my sensational experience at a given moment, I might well forget to name the sensations of pressure from my clothing simply because they are so constant, so presumably for a similar reason introspecters often fail to name the self.

(2) A second reason why controlled introspection has so little to say about the self, is that it has been largely occupied either with perception and imagination or with thinking—with the 'thought-processes,' imageless thought,' 'relational elements,' *Denkthätigkeit*. But perception and imagination have impersonal and 'external' objects and thought is mainly concerned not with personal objects—myself and other selves—but with strictly impersonal relations. When I study discrimination, comparison, or inference I am far more concerned with the impersonal relations, likeness and causality, or with the similar and the causally related things, than with my relating self and its fellows. In recording my introspection of perception, imagination or thought I am therefore very likely not to name the unemphasized consciousness of self.

(3) Directions given to introspecters seldom make mention of the self. The subject is bidden to attend to all his sensations—and he is especially reminded of the kinaesthetic and organic sensations—he is told to make note of pleasantness and unpleasantness if they occur and to mark the intensity and order and duration of the different experiences; and he may be asked to mark the appearance of any unsensational factors. There are even a few recorded instances in which the subject has been (unavailing) directed to note any case of self-consciousness should it occur. But specific sugges-

tions of phases or factors of self-consciousness are very infrequently given. The subject is seldom bidden to distinguish between receptiveness and activity of the self, or to note any awareness of sharing others' experience, or to observe any consciousness of himself as persisting or as differentiated from other selves. In default of such concrete suggestions he is bound to record his introspection in the terms most familiar to him—those of selfless psychology.

(4) The most significant reason for the infrequent appearance of the self in introspective records lies in two allied preconceptions about introspection which arbitrarily and by definition limit its scope. The first of these is the doctrine that introspection must have for its object either structural elements or psychic functions<sup>74</sup> and that a trustworthy report of introspection, in other words, a description of experience must limit itself to an enumeration of these elements (or functions). In the Cornell laboratory, for example, the observer is formally taught that "in introspection we are describing a conscious process" and that "the categories of description are the last terms of analysis, the elementary processes and their attributes."<sup>75</sup> Evidently the well-trained subject, taught that introspection consists in an enumeration of elementary processes (whether these be conceived as sensations, affections, and possible thought-elements or as functions) will report these processes only. The self is accordingly foredoomed to perpetual exile from psychology.

To the contention of the self-psychologist that this conception of psychology is inadequate and that a complete description of experience must include not only this enumeration of elementary processes but also a further account of the self whose the processes are, the upholders of this view of introspection oppose the second of their *a priori* dogmas. They declare that an account of the self in its different attitudes<sup>76</sup> toward its environment is a mere untechnical narra-

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<sup>74</sup> Functional, as well as structural psychology, sometimes dispenses with a self.

<sup>75</sup> "The Schema of Introspection," this JOURNAL, 1912, 23, pp. 494<sup>3</sup>, 495<sup>2</sup>.

<sup>76</sup> I use this term to designate the basal relations of the self to its objects. To the criticism (cf. E. A. Gamble, *op. cit.*, p. 197<sup>1</sup>) that the term should be devoted exclusively to the translation of Marbe's *Bewusstseinslage* I can only reply (1) that I used the word 'attitude' in the same year, 1901, in which Marbe employed *Bewusstseinslage* (cf. my "Introduction to Psychology," first edition); (2) that, though Marbe uses the term in a wider sense, my 'attitudes' would, in my opinion, fall under his *Bewusstseinslagen*; and (3) that I know no other term which expresses my meaning so well.

tion of experience, a 'statement of meaning' or a 'verbal statement,'<sup>77</sup> not a 'scientific description.' The acceptance of this dogma again automatically excludes the self from psychology since the everyday observer is constantly describing himself declaring, for example, that he can feel 'himself' growing angrier and angrier; that he is sure 'he' has been here before; or that he can make his goal if only he exerts 'himself.'

The doctrine of the radical difference between the object of the scientific and that of the every-day consciousness is stated by Titchener in the following passage: "The world of things and people . . . is never identical either with the world of physics or with the world of psychology; for physics deals not with boats and trains but with masses and distances and velocities; and psychology deals not with quarrels and successes, but with emotions and voluntary actions."<sup>78</sup> Few physicists or psychologists will find themselves in full harmony with this teaching. The world of physics and the world of psychology are, to be sure, not completely identical, each for each, with the world of things and the world of people, and physics and psychology, as analytic sciences, do indeed concern themselves with masses and velocities on the one hand and with emotions and volitions on the other. But this is far from a proof that physics and psychology do not analytically study the objects of the plain man's uncritical observation. Rather, the masses and velocities, the emotions and the volitions are the outcome of the scientists' analytic study of boats, trains, quarrels, and successes. To state this in more general terms: The argument on which is based the view that the object of scientific description can not be identical with that of the every-day consciousness is the fact that scientific description differs from the every-day narration of experience. This difference unquestionably exists but is amply accounted for by the difference in method and purpose between the scientific and the unscientific report.<sup>79</sup>

<sup>77</sup> The first phrase is used by Titchener, the second by Dürr. Neither phrase is unambiguous. The second unduly limits the meaning of its terms: even a scientific description is a verbal statement in the ordinary sense of the words; and the same comment may be made on von Aster's 'communication' (*Kundgabe*). As for the term 'meaning:' its different uses in contemporary psychology certainly call for comparative study. Between Titchener's use, for example, and Angell's virtual application of the term to 'thought processes' there is a contrast which should at least be named.

<sup>78</sup> "Description Vs. Statement of Meaning," this JOURNAL, 1912, 23, p. 167<sup>2</sup>.

<sup>79</sup> Cf. pp. 500ff. above.

There seems then no reason for accepting the dogma of the essential difference between scientific and every-day objects. And indeed this distinction is sharply contradicted by the actual procedure of the sciences which are forever analyzing and explaining precisely the objects—lightning and rainbow, tide and dew—which the plain man uncritically observes. To illustrate this from sense-psychology: the plain man has not been trained to notice the blueness of the afternoon shadows on the snow; but the psychologist, while correcting his description, does not refuse to believe that the plain man has seen an object roughly indicated by the term 'snow.' Similarly, though the psychologist may well question the accuracy of the plain man's account of the self he is unjustified in overlooking the common assertion, or implication, of a self basal to perceptions, memories and volitions. But whether or not the self-psychologist's positions are accepted, the main contention of this section is incontrovertible. For it is clear that if introspection be defined as enumeration of 'conscious processes,' and if description be limited to objects radically different from those of ordinary introspection, then the self is excluded, by instruction, from experimental report.

In conclusion, the remark may be hazarded that in view of the relative paucity of introspective studies and of their preoccupation with relatively impersonal experiences, and in view, also, of the directions given to introspectors and of the preconceptions on which these directions are based—it is perhaps more surprising that the self has played any rôle at all in technical psychology than that many psychologists should fail to record its presence.

## GRASPING, REACHING AND HANDLING

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*Mechanism for Grasping.*—The child is born with an automatic grasping mechanism which “goes off” when the proper place is stimulated. The trigger part of this mechanism at birth is most sensitive at the central palm. However, a stimulation of any part of the inner surface of the fingers may create grasping movements. After a few weeks the fingers become decidedly more responsive to grasping stimuli. On the 52nd day the baby herein studied grasped by his fingers firmly enough that one could pull him up by them, and 20 days later he would make grasping movements when even the back of the end sections of his fingers were touched. For several weeks, beginning near the end of the third month, while nursing, he would automatically grasp the round of the back of the chair in which his mother sat. Thereby he received painful injuries when she, forgetful of his habit, would hastily rise. Sometimes the chair was tipped, for the first effect of the consequent pull upon his arm was a tightening of the grasp.

The stimulus for grasping was evidently of the kinaesthetic sort and not that of touch. When at first the hand closed as the palm was touched it was a leverage, vibratory, sort of reaction. Probably the consequent stretching of the muscles of the palm stimulated them to multiplied contraction. The slightest vibration of pulling against the fingers stimulated grasp and the greater the pulling the stronger was the grasp. Certainly it was not a real touch sensation that immediately set off grasping. The human agent gradually learns, by trial and error, to use this wonderfully accurate mechanism in the various forms in which it develops during a life time.

*Thumb and Finger.*—From the first Baby's thumb closed outside of his fingers, but in opening the hand, the thumb tended at first to come in line with the first finger. Gradually from that time the thumb was extended more and more when the hand opened, up to the 195th day, when in reaching to grasp, the thumb was almost at right angles to the fingers. On the 272nd day, we dangled two spools on a double cord

in front of his face. The cord was seized at each end by the thumb and first two fingers, with most of the weight on the thumbs, which, with the hand moved in and out, toward and away from each other. Up to the end of the year when an object was grasped it was seized with thumb beneath, fingers on top. Probably this is nature's way of strengthening the thumb for the place it must have in the child's later experience. The fingers during the early months often made a variety of random movements, independent of one another. Frequently then any one of the fingers of either hand moved up and down while the others were perfectly still or, a finger remained still while the rest of the hand randomly closed. However, in seizing an object, there was perfect unity in the finger movements and by the 180th day the fingers tended always to move in line with one another. The development of the use of the index finger will be discussed below and the picking up of small objects between thumb and fingers will not be discussed in this paper.

*Field of Grasp.*—Practically from the beginning, and very decidedly by the third month, the hands, as a rule, moved within a small radius below his chin and often came to rest with palms half closed and vertical to his chest. In this position they often grasped each other, and the closer they were to each other the more unified appeared to be the movements. Therefore as any object came within the inward sweep of the hands it was grasped. When the hands gradually became independent of each other, this inward sweep of each hand still persisted, so that the field of grasp for each hand, at any moment, for several months was determined by the distance of the palm from the objects directly facing its surface.

There was such a gradual development of reaching for things that, even with the most careful observation, one dares not attempt to locate it on any one day. However, Baby apparently reached for the first time on the 94th day with a definite purpose, for a rattler held in front of him. The object was in the natural sweep of the hand and he moved his left hand in toward it, with the right following close behind. He focused his eyes upon the object as he reached for it. It should be noted that parallel with and prior to, the advancing field of grasp the focal accommodation, and consequently, the visual field developed. Four days later he readily grasped small objects when they were placed between his hands as the latter comfortably rested a few inches apart, about the lower part of his chest. On the 104th day

he grasped an object a few inches above his chest as he lay on his back. He also moved his right hand across his body just left of his breast bone and with eyes focused on my face touched my face; but it took considerable time to make the reach. Three days later he reached out several inches from his breast bone on the same side and grasped his rattle, and later vainly attempted to grasp my hand just above his face.

Even on the 109th day attempts to get either hand several inches across the median line of his body, were attended by much effort and not much success. On the 131st day he discovered the bonnet on his head and stretched it down over his face; he repeated this several times during the day. He dropped his rubber dog a few inches from his mouth, to the right, on the 168th day, and seized it with his left hand. He did not attempt to grasp objects by the hand of the side nearest the objects when they were very close to his body. On the 269th day when a bracelet was hung on his ear he reached vainly for it with the hand of the opposite side. Forty days later, when a small strip of cloth was put on his head so that it hung down between his eyes, he failed to get it after repeated trials, although he had long been able to pull a bean bag from the top of his head. Aside from these exceptions, however, by the middle of the year he could readily seize objects within the sweep of either hand, so long as volar movement was not required; and toward the end of the year, the body gradually tended more and more to aid the reaching arm in getting nearer the desired object.

*Scratching Movements.*—Practically from the first a stimulating of the ends of the fingers set off "grasping movements;" since the hands were often about the face, and were not very tightly closed, the finger tips often scratched and clawed, drawing blood on his face. On the 151st day he discovered that he could make a noise by opening and closing his hands when his finger tips touched my collar. After that time for several months he repeated this a number of times. When he felt over one's face he at once began a clawing that was unbearable. Opening and closing his empty hand with grasping movements did not begin, however, until the 285th day. By the middle of the year there was a wonderful modification of his reflexive grasping and clawing. He began then to softly feel over one's face with an attentive touch. Probably, attention to the touch sensations automatically inhibited. From the earliest weeks he put his hands out on his mother's breasts, but never once

clawed them. Probably the kinaesthetic sensations were the inhibitive stimuli. A few months later, although he would scratch mother's face at any other time, during nursing he would never scratch it. With fingers in a fixed clawing position, he began scratching objects with arm movement, apparently for touch and sound, on the 185th day.

*Use of One or Both Hands.*—During the first few months, when both hands partly closed were customarily near each other, palm to palm, either was apt to move toward the other when one was stimulated by an object for grasping, and long before he voluntarily grasped or reached for things, one hand grasped part, or all, of the other hand; at times parts of either hand were at the same time grasping the other hand. When an object was put near either hand the other hand invariably seized it or the grasping hand. But contemporaneous with voluntary grasping, during the early third month, developed independent reaching by either hand. For example, on the 94th day he put his rattle with his right hand into his mouth while his left hand lay motionless at right angles to his side. While there seemed to be no preferred hand save perhaps a little predominance of the left, by the 198th day, he almost invariably reached for things with the right. But the 114th day he would frequently grasp only with one hand and 24 days later he reached out one hand alone. He almost exclusively reached out with the right hand for things outside of easy grasping radius by the 183rd day. Since his wrists were not turned, objects back of his hands or, beyond the backs of his hands, could be grasped only by the hand of the opposite side and the right hand could reach much farther than the left across the median plane.

If by the 198th day, both hands were free, he would reach with both for things slightly beyond his grasp; but the right hand was always foremost and would reach farther than the left. He fairly "dived" for things and invariably reached with the right hand. By the end of the year he was more nearly ambidexterous in reaching. On the 343rd day, for example, he picked up a rubber ball by the right hand 36 times and by the left 12 times, while in all times but two it was thrown by the left hand.

*Holding Things.*—As with all his activities, so with grasping, attention toward the movement intensified it, and attention away from the movement weakened it. Therefore, the wide variety of new sensations he received upon beginning to grasp objects, meant that he tended to drop an object soon



after grasping it. Forty-five days after his first voluntary grasp he held his rubber dog as he looked at it, for the comparatively long period of one and one-half minutes. After about twenty-five more days he seemed to be able to hold a toy as long as he wished. Up to about the 170th day he would hold objects only by both hands; then he began to use one hand only, but he would hold the vacant hand near and in a position for grasping. On the 225th day he held and chewed a chicken bone for fifteen minutes without once dropping it and 75 days later he clung to an orange for about the same time, while he pointed to objects with the other hand. Then it was taken from him, and after it was given back, he held it for over one-half hour more. When angry and hungry on the 311th day, a graham wafer was given him; he neither ate nor dropped it, but held it in the same hand 50 minutes. Several times when ill, or when his feelings were hurt he would seem to get comfort from seizing a choice object. During a night of teething (348th day) he was given his rubber ball and he held to it for a half hour before falling to sleep and ten or fifteen minutes after.

*Errors in Reaching.*—For a few months casual observation shows that in reaching, he made errors of from a few inches to a foot or more. On the 163rd day he seemed to grasp at something in a window five feet away. However, I never saw him make any such error again. A number of tests made only four days later gave an error of no more than 12 inches. Then when an object just within his grasp, as shown by a large number of tests, was moved directly away from him he would hold out both hands as for grasping until the object was 10 or 12 inches away, when he would drop his hands, each to their respective sides. Likewise when the object approached him he would not extend hands until within about the same distance. This experiment was repeated on the 189th day when the error had decreased about one-half. From that time on his estimation of distance away from him, gradually became more accurate.

*Grasping His Feet.*—Baby discovered his feet on the 141st day and while on his mother's lap held his left foot with both hands; but the pleasure of the new experience caused him to extend his feet quickly and he lost his hold. Two days later he made two vain attempts while on his back on the bed, to seize his feet as they were kicked up. The left came up higher than the right. This kicking up of feet which developed almost instantly with his attempt to reach his feet

was not a purposive movement, but when he would reach forward toward his feet, thereby raising his shoulders considerably, the contraction of the muscles that brought up the feet was merely overflow innervation, such as is common in the development of any voluntary control of other parts of the body. On the 150th day he kicked up both feet and with both hands seized the left foot, but immediately lost it and failed in repeated trials. Six days later in the same manner he reached both hands for his respective feet, but succeeded only with the left; then he turned the right hand to assist his left. On the next day without looking at them he seized each foot by his respective hands. He could do this with wonderful precision and without looking at them, whether his feet moved wide or close. Since he could kick up his feet only when lying uncovered all the possible reactions were recorded. Therefore the wonderful improvement without intervening practices is very significant. On the 197th day he pulled off a shoe twice, and the next day he succeeded, after 15 minutes, in pulling off both stockings.

*Touch.*—Although from the first he experienced touch sensations from any part of his hands, there was no evidence of specialized touch organs at his finger tips, as in adults. However, the end organs of touch seem to have been deep, or faint, at least, in response. Probably from the beginning he got a great deal of touch sensation from his grasping process, but as late as the 152nd day my notes state that "while he was eager to drum over new objects, fixed surfaces, etc., and while he would bend forward in one's arms to thus 'touch' objects it was a hasty rubbing, drumming, grasping touch." By the 225th day there was an inordinate desire to touch things, expressed in squirms, twists, wriggles, and jumps of the trunk. Although the rubbing of the hand over objects was still heavy and hasty, by this time he would always spread his fingers wide apart, with thumb far from fingers while feeling over flat surfaces. This continued for at least a month.

Not until about the end of the eighth month did gentle, careful touching appear. Then he began to softly touch one finger of one hand by another finger of the opposite hand. With this kind of procedure the index finger predominated in touching. In a few weeks, he began examining more things, first by the index finger then by all the fingers. As will be described later the index finger always touched small objects before they were seized between it and the thumb. Even by the end of the year the finer touch sensations at the finger tips, such as an adult possesses, were comparatively faint and

seldom were voluntarily experienced. It might be noted that during the third month, when before voluntary grasping he got the finger tips of the hands softly together, he seemed to be getting touch experiences. Perhaps the chief results, however, were vibratory kinaesthetic sensations instead.

*Putting Things into Mouth.*—As soon as he was able to put his hands into his mouth, objects put into his grasp were carried there too; he naturally continued putting things into his mouth, after he had developed voluntary reaching and grasping. By the 252nd day objects too heavy for his grasp he rolled to his mouth, and for the first time, a new object was not immediately put into his mouth when given to him. Two weeks later he would almost invariably drum other objects with the new object before putting it into his mouth.

After touching a lettuce leaf cautiously with his index finger (305th day) he put it into his mouth, and less cautiously, a plantain leaf and a carnation. Two days later when a bunch of carnations was presented to him, without pausing to look at them, he put them into his mouth. It is obvious that the pleasure of reaching and grasping an object was far superior to that of looking at it. Toward the end of the year he became less likely to put foreign substances into his mouth and would generally give such things to his mother at her request; furthermore he offered little resistance to things being taken from his mouth. While in the first months any new object went first of all to his mouth, at the end of the year he used his mouth last as the medium of getting acquainted with an object.

On the 252nd day he for the first time released grasp of an object which he had put into his mouth, but held his hands as if ready to catch it. During the tenth month he began to grasp objects by his mouth and then to creep with them. On the 325th day he dragged his mother's shoe across the floor between his teeth, and when he could merely support his body in a creeping position on each hand several months before, he seized a newspaper by his teeth and shook it as a terrier shakes a rat; several times during creeping he seized a ball thus between his teeth.

*Reaching Objects with Hand Holding an Object.*—About a month after he began reaching for things, he started to reach for objects with the hand already grasping an object, while the empty hand was idle; for several weeks the seizing hand tended to reach for the new object. Probably the stimulus from the grasping of the first object spread to the arm thereby

and made the arm more ready to advance. Furthermore, the interest naturally directed to the occupied hand may have made it more prone to act. Added to these factors may be the effect of the new experiences just now developing of touching things with things.

This last factor cannot be the prime cause; for on the 189th day with the rubber dog in his right hand he reached again and again for his spoon, each time touching it just once with the dog, but not releasing his grasp. Of course the vibration thus caused would stimulate rather than help release the grasp. After repeated trials he put the dog into the left hand and reached slightly with the left; but after a little hesitancy he grasped the spoon with the empty hand. Even up to the 285th day he would often extend his occupied hand rather than the empty one toward objects offered him. Occasionally he would change the object in his hand nearest to the desired object to the other hand before reaching; but never up to this time did he immediately drop one object to take up another. Dropping objects will be rather fully discussed in a later chapter.

*Touching Things with Things.*—Contemporaneous with his reaching, developed his touching things with things. For example, on the 201st day he touched his mother's eyes with a spoon which he changed from one hand to another a number of times. He made no attempt to touch her eyes with empty hand. By the 210th day this desire had become so strong that as soon as a spoon was given him he reached it out to touch the nearest object. On the 331st day he found a broom splint on the floor and for ten minutes crept about the room touching things with it and smiling. Aside from the enlarging of his general field of activity, this touching things with things gave him a projected touch sense which was new and attractive.

*Reaching Distant Objects with Long Objects.*—In reaching for things he gradually learned from experience that he could touch things by an object, such as a spoon in his hand, which he could not reach with his empty hand. Likewise, after several smooth round sticks, varying from 6 to 14 inches in length, had been given him, he learned in his desire to hit things with them, that he could reach objects he could not reach with his shorter toys. For example, he learned on the 192nd day, that he could more easily reach the front of the carriage by the aid of his spoon; and 9 days later, after failing to reach his spoon with the empty hand (by his natural

inward movement) he tried to scrape it toward him by the aid of his rattler. When things on the table were removed beyond his grasp he hit them with his spoon. After repeated efforts, on the 259th day, to reach objects on the table, he seized a long round stick lying on the tray of his high chair and touched them first, then hit them.

*Hitting Things with Things.*—On the 198th day he held a piece of paper in each hand and by hitting them together made a rattling noise. The movement of the hands increased as the sound grew louder. At the beginning of the 7th month he held up a rattle doll in one hand and hit it with the spoon in the other. When we gave him a pan and a powder box on the 233rd day he took one in each hand and struck them together. In this movement there was always a preferred hand, i. e., one hand tended to move farther than the other. This tendency to hold one object partly or wholly still while the other did the hitting gradually gave place to equal movements for both hands. However, as late as day 254, he held a small block in the left hand and drummed it with a similar block in the right. He was as likely to hit one on the head as to hit any other object. By the 266th day he had developed a habit of striking a new object on another object before examining it with his mouth. However, on the 278th day he had a new block 12 min. 35 sec. before he began to drum with it. Up to the end of the year he often amused himself at various times by striking very hard with his toys on pans, boxes and metal bath tub.

*Moving Things by Things.*—By so frequently touching things with things in his hands he accidentally learned that he could move the touched object; and so, by the beginning of the eighth month, he would scrape objects beyond his grasp off the table by his spoon, making movements always toward him. On the 256th day he amused himself for over half an hour, scraping blocks and toys off the table by a coat hanger. He used the hanger to reach those within easy reach of his empty hand as well as those more distant. Having accidentally rolled one large bottle against another, he continued for 11 minutes to roll the second before the first, on the 325th day; six days later he rolled a ball by pushing it with a small block as he crept after it. He also rolled a ball by the remote end of a stick from a window shade.

*Handing Objects to One.*—For the first time, on the 299th day, he put the stuffed dog which G dropped, into her extended hand. Of course he did not release his grasp until

she seized the dog. Then without her request he handed it up to her from the floor to the couch seven successive times. Five days later, whether I asked him or not he would hand the ball to me. On his own initiative, after he had repeatedly knocked down the blocks (day 313) from the top of the milk bottle where I had built them, he handed them to me each time after he had knocked them down. With the beginning of the tenth month he started voluntarily to hand one small object such as a rubber band or splinter with "eh." He would readily release grasp but would not of his own accord drop them into one's hands. This readiness to release objects increased up to the end of the year.

*Pushing Things Away and Reaching for Them.*—After he began to sit alone on the floor, and especially after he could creep, he would push objects away from him, then reach for them. On day 278, after playing with a new block for 5½ minutes, he scraped it back and forth on the floor, pushed it away and then reached for it again and again, but after a number of failures I got it for him. He at once pushed it away again and reached for it. Forty days later he amused himself for several minutes with a slipper stretcher. He put it on the seat of a rocker, pushed it out of sight and released his hand; he reached for it again. Then for several weeks he showed a marked tendency to put things out of sight, drop toys behind the couch, push them under it and reach for them. As standing and walking developed this tendency gradually disappeared.

*Creeping for Things and With Them.*—Of course his creeping began in his desire to get desired objects; and as the first creeping was attended by great effort, and was very slow, he naturally reached for an object as soon as he was near enough. This tendency to reach for things on creeping for them, persisted even after he could move rapidly, to the end of the year. During the first few weeks of this procedure he invariably used his right hand in reaching for the object, pushed his body up and swung about on left hand to face one. When he secured the object he would always get up to a sitting position at once, and manipulate it with both hands. By the tenth month, he gradually grew more ambidexterous in reaching for things toward which he crept. However, the right hand always predominated to the end of the year.

After he had crept for about a month he began creeping with objects in his right hand. The object would strike the

floor with each creeping movement of the seizing hand. He crept thus the whole length of the room with a ginger ale bottle but he merely slid the bottle along between the "steps" while the seizing hand made regular strokes. Soon he was thus traveling over the floor with various objects. Up to the end of the year the left hand was seen but two or three times carrying an object when creeping and then only very small objects.

*Extending Both Hands.*—When he first reached to seize one's extended hands (145th day), both hands came in unison; by the 156th day with both hands he seized his respective feet without looking at them. Even when by the 177th day, he would extend hands without one's first suggesting it, both hands were offered. Furthermore when he wanted one to take him he extended both hands, but never during the whole year did he reach for two objects by both hands at once, although he practically always extended both hands to seize a new single object, almost to the end of the year.

For the first six months the extending of the hands was momentary, with one impulse, and would often be successively repeated. The sustained holding out of the hands did not appear until about the 198th day: then for 15 seconds he held out both of his hands to his mother until she took him. Likewise, toward objects desired his hands were not held out sustainingly until the 256th day, but were jerked upward and forward on one impulse, that was frequently repeated successively. Gradually only one hand was used to extend toward an object of interest, and with this development came a sustained holding of one hand toward the thing desired. The palms were always down, fingers and wrist a little curved. This pointing toward objects with the hand developed into pointing with the index finger which was first apparent on the 296th day and became quite pronounced by the end of the year.

*Volar Movements.*—During the first year the wrists were very rarely turned. The palms were practically never seen upward, save while the hands were above or beside his head when asleep, in which position they were never otherwise. On the 108th day after 4 minutes' attempt to grasp the pillow against which the back of his hand rested, he failed more than to rub the back of his hand over it and to scratch it with his finger tips as he bent his hand slightly backward. He was once seen, however, on the 129th day, with ends of

fingers of one hand clasped by those of the other hand, whereby a partial turning was effected by each wrist. The complete turning of the wrist was first in evidence near his mouth, when on the 125th day he completely turned his palm about to grasp a toy I held near his mouth. Heretofore he had merely lifted up the hand with its back toward an object near his mouth, and had sometimes pushed objects into his mouth in this manner. Inasmuch as so many things were put into his mouth, it is natural that he tended to develop the regular turning of the wrist in grasping and in holding objects near or in the mouth. Furthermore, with the adult, in the natural sweep of the hand toward the mouth the palm tends to roll slightly toward the upward position. After scratching a cushion which the back of his hand touched, he lifted up his hand a few inches and rolled it about with palm toward cushion. He clearly turned palm about to seize a basin in two successive trials on the 188th day. During the early sweep of the hands toward each other, just on the threshold of grasping and in all such movements during the rest of the year, the palms have been vertical. However, during the whole year, aside from the above noted exception, he was never seen to grasp a toy with palm upward.

*Strength Development of Hands.*—As records of other children show, the strength of his grasp at birth was phenomenal. A spoon seized on the first day could be released only by bending his fingers by one's own hand, and by the 153rd day he could hold himself up as well as pull himself up, by my grasping fingers hooked over his. However, after 25 more days, his fingers did not sustain sufficient firmness to thus pull himself up; his general grasp control had been developing, nevertheless, and on the 196th day, while on his back, he grasped a medium sized orange on his chest and raised it, clear of his body, to his mouth. He lifted a small orange to his mouth by his right hand, on the 224th day, and a medium sized Florida orange with either hand on the 254th day. On the 282nd day, a gas iron of  $5\frac{1}{2}$  pounds was placed beside him on the floor; he tried to pick it up but he could only tilt it over, push it along on its side, and tilt it back again. Forty days later while in a sitting position he lifted it clear of the floor. He tossed a pound of beans about by either hand on the 287th day and he readily lifted my large umbrella by one hand, on the 295th day. Eleven days later he lifted a quart milk bottle by either hand, and on the 326th day he put the milk bottle into a rocking chair.



He could lift his mother's shoe above his head with either hand by the 313th day and the next day he could slide a volume of Ladd and Woodworth's *Physiological Psychology* and one of James' *Principles* over the floor rather easily. Near the end of the year he could slide a chair about the room, either from his feet or his knees.

*Handling Things.*—When I put a rattler into his hand (70th day), he grasped and rattled it in an irregular manner for a few moments; then it dropped. It was not a purposive movement but it succeeded a pleasurable feeling which feeling was beginning to find expression in movement of the arms. Even on the 198th day, when given some bells, he did not rattle them by quick jerky movements. In fact that type of hand movement had not developed and he moved them back and forth in the right hand, rather gradually, describing an arc between his shoulder line and sternum. Then the bells were moved from hand to hand, and occasionally chewed. Toward the last of the 8½ minutes during which he held the bells, he moved his hands faster, both in moving the bells and in exchanging them from hand to hand. Probably this was expressive of a higher degree of pleasurable feeling. He proceeded in about the same way with a piece of paper, save that he made quicker movements and changed hands more frequently. By the 211th day he used his rattler in a different way from that of any other toy. He would shake it with quick jerky movements, and bring it around and flourish it in the air. He had learned to shake his bells effectively by the first of the eighth month; the most attractive object then was a piece of paper which he could tear, drum, rattle, crumple and, if permitted, chew. By the 256th day the shaking of bells and rattlers had gradually changed from a quasi-lateral movement to a movement almost directly up and down. This latter type persisted to the end of the year.

Instead of chewing a piece of cloth as a towel or a diaper within his reach, by the 268th day, he seized quickly and waved it with 2 or 3 jerks of the hands, when it was dropped.

*No Interference with Feeding.*—During the first weeks, when peppermint or Castoria were given him, he made reflex striking movements from his mouth outward; on the first day he automatically grasped the spoon with which the nurse was feeding him. Later, when we gave him warm water for colic we necessarily developed the habit of holding down his hands. On the 190th day, however, his hands were not

held, and when the first spoonful of water was given him he reached for the spoon with both hands; but when the second spoonful was raised he made no interference whatever, and none up to the last and fifth spoonful. After that he continued not to interfere with the spoon when given water to drink, nor with the cup, when he had learned to drink from it. At the end of the year (363rd day), when one put a cracker or a bit of breadcrust to his mouth, he would not raise his hands at all until one put in a large piece, and then merely to hold the morsel in his mouth. This is a striking example of the inhibition of strong movements as a result of attention away from the movement.

*Inhibitions.*—Feeling over my face, 187th day, he accidentally grasped my nose which he squeezed gently between his thumb and first finger; he did this again and again with wonderful precision. Four days later, he felt over my face carefully and softly, with no clawing whatever. He reached in the following ways toward a watch on the table with both hands on the 234th day (a) he inhibited left about half way; and seized with right; (b) he reached out left almost touching, but seized with the right which had moved just behind the left; (c) same as (b); (d) almost touched with left, but seized with right; (e) same as (d); (f) he took the watch with the right hand directly on final trial. In the first two cases the new experiences so attracted the attention that the regular movements did not occur, for attention away from a movement always minimized or ended it. The last case, however, is not so simple. Toward the end of the year after his hands had been slapped a few times for seizing prohibited articles he would often check reaching movements before their completion. At first the signal for such inhibitions was one's prohibitive tone, later a mere gesture or look, and finally no signal was apparently needed. Probably it was the emerging in memory of a past experience.

*Emotional Movements.*—Although a whole chapter will be devoted to "motor emotional movements," a few words are necessary here. During the first few months any attempt to move his hands, as in "Patacake," was met by resistance and the arms and hands tended to grow taut; but by the 164th day he released tension of his hands perfectly, for a few strokes of "Patacake," and opened the palm of his left hand almost entirely, while the right was a closed fist. Five days later the left hand opened perfectly in "Patacake," but the right only about half way. On the 182nd day he opened his

right hand fully and flat, as his mother slapped her cheek with it on the third stroke, but after a few more strokes he closed it; he did the same with "Patacake."

When manipulating objects (275th day), while in a high state of pleasure, he generally moved the empty hand in concert with the grasping one. This was especially true in the ringing of his bells. These unified movements occurred in series of from two to ten. Gradually this same type of movement by both empty hands developed, and at the close of each series, the hands tended to come to rest, clasped together palm to palm, in front of his body just below his chest. At the close of a series of these unified movements, on the 304th day, he made two successive audible hand claps. Seven days later, when one clapped Baby's hands saying "Patacake," he relaxed both hands freely, so that one could make a slapping noise. He slapped his mother's breast of his own accord, so as to get a loud sound. Hand-clapping soon became voluntary, and, during the last month and a half of the year it was a frequent expression of a high degree of pleasure. Herein is seen the genesis of applause.

During the last few months of the baby's first year his hand movements were employed chiefly in creeping and the development of walking.

# THE INFLUENCE OF SUGGESTION ON IMAGINATION

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*Introduction.*—It is generally known that the imagination may be set working along certain paths by suggestive influences. These influences may be either internal or external. They are internal when they take the form of a recollection or organic sensation. They are external when they are initiated from without—when they reach consciousness in the form of an idea, perception or sensation externally induced. Thus a man may cross a bridge before he comes to it, but not till something in the road suggests the idea or reminds him of the bridge ahead. Or one may hear the clang of fire engines and thereupon begin to picture to one's self an exciting fire-scene. A mere word, a phrase or name on a printed page may also suffice to switch the mind over to a passive train of thought. In this sense it is possible for an external situation to arouse a particular train of imagination. This being so, the question arises: first, to what extent can external influences start the imaginative process; and secondly, do they increase or decrease the range or fertility of imagination?

It is obvious that these questions have a significant bearing on certain sociological and pedagogical problems. Imaginative ideas are the threads out of which the texture of active life is spun. A considerable portion of the pedagogy of play is based on the above proposition. It is assumed that by skillfully controlling the child's games, as by placing certain toys in his hands, the kind of ideas that shall occupy his mind can be determined beforehand. These ideas gradually form a habit of thought; and this, of course, is the basis of every career good or bad.

It is out of a desire, therefore, to discover (1) To what extent can suggestion control or direct imagination? and (2) Is suggestively induced imagination more or less fertile than free imagination? that the following experiment was undertaken.

*Method of Procedure.*—The advisability of using ink-blots in tests of imagination has already been demonstrated by G. Dearborn, Kirkpatrick, Sharp and others.<sup>1</sup> In this experiment, therefore, these blots were used under the following conditions: In one instance five ink-blots, properly marked, were distributed among five individuals. The observers were told that on a given signal each one was to pick up the blot before him, hold it in one position, and endeavor to see how many things or objects he could discover in it—"in the same way as one sees things in clouds." The time allowed for looking at each blot and writing down the results simultaneously, was one minute. Then the blots were passed from one subject to the other till the five of them had made the round. In this manner twenty-five judgments were obtained in about fifteen or twenty minutes.

After that another set of ink-blots was distributed among the five observers, but this time each blot was accompanied with a picture post-card. The subjects were now given the following instruction: "You have before you an ink-blot and a picture post-card, each of them turned upside down. At a given signal you are to look at the picture post-card for 15 seconds and try to saturate your mind with its contents. When the 15 seconds are up, I shall say 'down;' and you are to turn the post-card down again and try to think of its contents for 20 seconds. At the end of this interval I shall say 'now' and you are to pick up the ink-blot, hold it in one position, and endeavor to see how many things your imagination can discover in it, as in the former case. This you will do for a period of one minute during which time you must also write down your results. When the minute is up I shall give you a signal to lay down your ink-blot, but you may finish writing your introspection if you happen to be so engaged at the time." This done the ink-blots were then passed in one direction and the picture post-cards in the opposite direction. In this manner each of the five blots was combined with each post-card, while no observer saw either the same blot or the same card more than once. Perhaps a simpler method would have been to keep the same ink-blot and picture card together all the time and have both of them pass from observer to observer. But unless a very large number of tests are made, this method may give rise to a serious error, i. e., it may be that some ink-blot should have a figure on it which corresponds to the accompanying picture

<sup>1</sup> See Whipple, *Manual of Mental and Physical Tests*, pp. 429-457.

post-card or some portion of its content. The possibility of such chance occurrence I tried to avoid for reasons presently to be explained. Needless to say the position in which the ink-blots were to be held was indicated by a mark and was uniformly maintained by all observers.

The object of presenting the ink-blots alone, as in the first instance, was to obtain a measure or an index of free imagination; for it is known that ink-blots thus presented serve to measure the fertility as well as type of imagination.<sup>2</sup> The reason for presenting picture post-cards together with ink-blots, as in the second instance, was to see whether the contents of the former exerted any influence on the imaginative products of the latter. If they did—if, for instance, the saturating of one's mind with the picture of a dog caused one to see a dog in the accompanying ink-blot more frequently than without previously looking at such a picture, then it would show that suggestion can direct the process of imagination.

Sometimes the tests were begun with controlled imagination first, i. e., ink-blots with picture post-cards were first passed around, then ink-blots alone. At other times the order was reversed. My observers, three of whom were women and two men, came to me once a week.<sup>3</sup> They did not know the purpose of the investigation at first, but toward the end of the third week they declared that they thought the object of the tests was to see whether they would discover the same things in the ink-blot that they had seen in the picture post-card immediately before. This, of course, is unfortunate; for it is difficult to say whether the product of the controlled imagination was genuine, that is to say, whether it was due to the unconscious suggestive influence of the picture post-card seen immediately before, or due to the wish and the conscious effort of the observer to see the very same thing in the ink-blot that had been seen in the preceding picture card. I instructed my observers that they were not to make a conscious effort to do that—that they were to give free rein to their imagination whether preceded by a picture post-card or not. All of them declared that they carried out my instruction—that wish or desire played no influence in their judgments.

In this manner I obtained two sets of results. The first represented free, or uncontrolled imagination. The second

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<sup>2</sup> Whipple, *Op. cit.*

<sup>3</sup> For participating in these tests, I thank Mr. and Mrs. I. L. Cohen, Miss Sarah Harrison, Miss Helena Rosenthal and Mr. Leon Silver.

externally controlled imagination, or imagination under the influence of suggestion. Let us see what the differences were.

*Treatment of Data.*—Certain arbitrary rules had to be laid down for the computation of the data. It is not enough merely to count the number of judgments rendered in each instance. The complexity of the judgment must also be taken into consideration. Thus a judgment like "A chicken" or "A dog's head" is not worth as much as a judgment like "A bird flying" or "A man riding a buffalo." Accordingly the judgments were treated on the basis of the nouns, verbs and descriptive adjectives they contained. The following table shows the frequency of these elements in the products of imagination with and without suggestion. They serve as an index to the fertility or range of imagination.

TABLE I

FERTILITY OF IMAGINATION AS DETERMINED BY COMPLEXITY  
OF JUDGMENTS

	Without suggestion	With suggestion
Nouns given.....	83	62
Verbs given.....	22	12
Descriptive-adjectives.....	17	11
Total.....	122	85

The number of trials in each case was the same, of course. My protocol further shows that out of the 57 trials to discover imaginary figures in the ink-blot under the influence of suggestion, only 14 judgments bore any relation to the picture post-cards seen immediately before. That is to say, the imagination was influenced and directed into definite channels by externally induced mental contents in 24.5 per cent of the cases.

On the other hand, the above table shows that in these 57 trials the imagination encompassed 122 ideas without suggestion and only 85 with suggestion. This means that there was a loss of 29.4 per cent in the range or fertility of imagination when it functioned under the influence of previously induced suggestion.

Moreover, there were 60 distinct judgments rendered in imagination without suggestion, and as these comprised 122 ideas it means that each judgment embraced on the average 2 ideas. There were 49 distinct judgments in imagination with suggestion, and as these comprised only 85 elements, or 1.7 per judgment, we have further proof that suggestion tends to diminish the fertility of imagination.

Another fact which indicates that suggestion or the attempt to direct the process of imagination diminishes the range thereof, is that out of the 57 trials with suggestion there were 13 nil cases, i. e., there were 13 instances when the observers were totally unable to discover a figure or any meaningful object in the ink-blot, whereas there were only 5 zero cases in the free or uncontrolled imagination.

Suggestion is usually defined as the "belief in an idea" or as the "acceptance of an idea as real and the dismissal of the opposite idea as unreal."<sup>4</sup> In other words, suggestion is the induction of an idea or motive or wish or impulse to motor activity which activity would not exist either in a potential or kinetic state under normal conditions. Therefore before we can say definitely whether those products of the imagination which correspond to the contents of the picture post-cards are really due to the suggestible influence of the post-cards or are mere coincidences, we must determine to what extent those products or products of the least resemblance to them, appear in the results of the uncontrolled imagination, i. e., under "normal conditions."

The following table represents (1) the cards—marked A, B, C, etc.—that "influenced" the imagination at least once out of 4 or 5 trials.<sup>5</sup> (2) The second column shows the number of times that an object corresponding to the picture post-card was seen in the 4 or 5 ink-blots associated with it. (3) The third column gives the number of times that a similar object was seen in all 57 trials of *free* or *uncontrolled* imagination.

TABLE II  
THE PROBABILITY THAT THE POSITIVE PRODUCTS OF CONTROLLED  
IMAGINATION WERE DUE TO SUGGESTION AND NOT  
TO COINCIDENCE

Suggestive cards that were effective	No. positive judgments in 4 or 5 trials	No. similar judgments in 57 trials free imagination
A.....	1	0
B.....	2	0
C.....	1	0
D.....	1	0
E.....	3	2
F.....	2	0
G.....	3	5
H.....	1	1

<sup>4</sup> Münsterberg, *Psychotherapy*, p. 100.

<sup>5</sup> At some of these tests only four subjects were present.



The foregoing figures show that there were three distinct occasions when the products of suggested imagination appeared in free imagination. But it should be borne in mind that the proportion in which they appeared in the controlled condition is much greater than in the uncontrolled. Thus one object (a dog) was seen 5 times in the 57 trials of free imagination. This is in the ratio of about 1:11. But the same object appeared 3 times out of 4 trials when the four observers first looked at the picture of a dog and then at the *four different* ink-blots. The ratio in this case is therefore about 9:12. Hence we may say that even this item, for the imaginative construction of which there seems to exist a natural proclivity, appears about 9 times as often under the influence of suggestion as in free imagination.

The description of a few cards and the products of the corresponding acts of imagination may show better than figures whether suggestion exerted any positive influence.

Card A: Yellow Easter card, three egg-shells, chicks crawling out of them. Object seen in associated ink-blot: "Contents of broken egg."

Card B: Christmas card, pine trees, horse and sled driving up road, two dogs facing horse. Objects seen in associated ink-blot: (1) Christmas tree, (2) Dog's head facing horse's head."

Card C: Easter lilies. Product of imagination: Flowers.

Card D: Two young dogs taking bath. Product of imagination: "Two dogs playing."

Card E: Pond with two ducklings on it, one swimming, other attempting to fly. Product of imagination: (1) Aquarium. (2) Bird flying. (3) Young chicken pecking.

Card F: Light house flashing in dark sky. Product of imagination: (1) Burning torch. (2) Light house turned upside down.

But the fact that the products of suggested imagination seldom appear in free imagination is not conclusive proof that they are entirely due to suggestion, for it is quite possible that the products of free imagination itself are equally seldom repeated. To settle this doubt I examined at random 20 products of the 57 attempts at free imagination in order to see whether any of them appears among the 56 remaining ones, and if so, how often?<sup>6</sup> The following table gives the results.

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<sup>6</sup> Actually there were 52 successful trials, since 5 of the 57 were nil.

TABLE III  
FREQUENCY WITH WHICH PRODUCTS OF FREE IMAGINATION  
WERE REPEATED IN ORIGINAL OR COGNATE FORM

Product No.	Times repeated in 56 trials	Product No.	Times repeated in 56 trials
1	9	11	5
2	2	12	1
3	1	13	1
4	0	14	9
5	2	15	9
6	9	16	1
7	2	17	3
8	0	18	5
9	9	19	7
10	9	20	0

Out of the 20 items only 3 failed to find repetition, whereas 7 of them were repeated 5 or more times in 56 trials. This shows that free or undirected imagination tends to move in a closed circle. In order to examine this phase of the problem more closely, all judgments of free imagination (nouns alone) were grouped in classes of kind or species. Then the judgments of suggested imagination were examined on the same basis. The groups into which they fell were as follows: (1) Man, (2) Bird, (3) Horse, (4) Dog, (5) Fish, (6) Plants, (7) Other animate things, (8) Other inanimate things. As there was repetition of both types of imaginative products in the first six classes, these were eliminated. The last two classes were then analysed. They contained (1) as products of controlled imagination, 21 distinct objects; (2) as products of free imagination, 23 objects, but as five of these were repeated two or more times, there were only 15 distinct items. This is further proof that free imagination tends to move in a limited area. That the extent of this area is determined by the mass experiences of the observers there can be no doubt. Dearborn makes a similar suggestion, when he says "that at least in mature subjects, the results of the ink-blot tests are conditioned . . . by habits of living, occupation, and other environmental factors."<sup>7</sup> In the light of these facts we shall have to revise the conclusion drawn from Table I; instead of saying free imagination is more fertile than directed imagination we shall have to say, it is more productive or more active, but its relative versatility depends upon the variety of the suggestive stimuli used in the controlled imagination.

The items that were repeated most often in free imagination

<sup>7</sup> As quoted by Whipple, *Op. cit.*, p. 434.

are, in the order of frequency, "man," "man on horse or horse alone," and "dog." It is advisable, therefore, not to use pictures that contain such figures as these as stimuli of suggestion. For if the resulting product of the imagination should correspond to the content of the picture, one would not be able to tell off-hand whether it was due to suggestion or to the natural tendency of the observers to imagine such items as "men," "horses" and "dogs" in ink-blots.

To remove that difficulty or anything analogous to it, recourse was had to suggestion-stimuli that were extremely particular and concrete in nature and none of which had appeared even once in the 122 products of free imagination. These stimuli were pictures of merchandise cut out of a trade journal. They included such items as an office desk, a bed, a clock, a chair, a stove, and so forth. Thirty tests were made with them, but only in two instances was there partial correspondence between the object imagined in the ink-blot and the figure seen immediately before. Moreover, 12 of the 30 trials gave nil results. That is to say, the observers found it very difficult to see anything at all in the ink-blots when they first saturated their minds with these merchandise figures. When asked to give their reasons, they declared univocally that the figures were too concrete, that they imparted no meaning outside of themselves. Whence it appears that the more particularized the suggestive stimulus the more does it narrow the range and productivity of imagination. This phenomenon, however, needs further investigation before it can be definitely established. Our figures, scanty as they are, seem to justify the following conclusions:

(1) Visually induced suggestion can direct visual imagination about 24.5 per cent of the time.

(2) Such suggestive stimuli tend to diminish the fertility (productivity) of the imagination about 29.4 per cent of its normal range.

(3) Such suggestive stimuli also tend to diminish the fertility of imagination as determined by complexity of individual ideas.

(4) The more particularized the suggestion the more does it tend to diminish the fertility of imagination.

*Concluding Remarks.*—It may be interesting to see what light these facts throw on certain educational and sociological problems.

In the first place, the question may be raised whether it is

advisable to direct the mind in a strict and narrow path? We see that positive results in "ideal construction" are obtained in only a fraction of the instances with previous direction, while on the other hand, mental power as measured by fertility is diminished. Does not this experiment bear out the oft-repeated statement that too much specialization narrows a man, diminishing his resourcefulness and making him helpless in the presence of new situations? Our results show that by controlling the constructive power of the mind more is lost in extension than is gained in intension.

This experiment also seems to support the general belief that the country raises the healthiest-minded children—the poets and presidents. The reason would seem to be something as follows: The ideational stimuli that impinge on the mind of the city child are too concrete, too particularized. Consequently the productivity of that child's imagination is diminished. The country child, on the contrary, receives suggestive stimuli that are vague and indefinite in form—such as broad plains, thick forests, unbroken skies, floating clouds, placid pools and running brooks. These stimuli, instead of acting like the picture post-cards—as directives of imagination—function rather like the ink-blots, i. e. they impregnate the imagination, giving it free play and inviting it to read into them whatever it pleases. Not so with the objects of the city—such as brick houses, clanging fire engines, shrill noises, chaotic mobs, rumbling wagons, grating trolley cars and buzzing automobiles—these things are too particular, too concrete in nature to invite the free play of the imagination. They function like the pictures of merchandise which I used in my last test. Apart from breaking in on the quiet play of imagination, owing to their multiplicity and inherent vividness, they lack the power to suggest anything outside of themselves.

But on the other hand, because of their multiplicity these stimuli awake in the mind of the city child a greater variety of distinct ideas than the country child ever experiences. Hence the apparent versatility of the former in contradistinction to the simplicity of the latter. Nevertheless, the suggested ideas, as we saw, are not as rich in complexity as those which result from free imagination. And this explains the general superficiality of the city child, notwithstanding his versatility, as contrasted by the deeper albeit simpler mind of the country child.

Again, the question arises as to who profits most from his toys—the rich boy whose playthings are the miniature reproductions of the concrete things that occupy the minds

of mature persons, or the poor lad whose toys, as a rule, hardly bear the least resemblance to the things which he makes them represent in his own childish life? According to the results of our experiment it would be the latter. For example, the rich boy who has a real rocking horse cannot experience half the range or variety of imaginative products that the poor boy who uses a broom stick for a horse must experience. The reason is that in the former case the rocking horse is too representative in form of a real horse, and this close resemblance must bring it to the consciousness of the youngster that after all it is a dead horse. Thus his illusion is spoiled. But the boy who uses a broom-stick for a horse, seeing no too great resemblance between the symbol and the object it stands for, is in no danger of being disillusioned, and so he gives himself up to the play of his imaginative ideas freely and enthusiastically.

Still the value of free imagination must not be overrated. For we have seen that its fertility is not always real, since it repeats itself and moves in a circle the radius of which is determined by the individual's experience and environment. The derivation of the best type of inventiveness or imaginative power, therefore, would seem to be a psychological problem in maxima and minima.

From a sociological point of view these results show most clearly the prodigious influence of environment on mental development. If ordinary picture post-cards can direct the process of imagination in about 25 per cent of the cases, how much more so must this be done by those active, living influences which carry an emotional complex with them? The sound of martial music or the break of the ocean waves, the presence of this or that type of man or woman, the association with business men or with scholars—any one of these or similar influences, if persistent enough, is bound to fill the mind with a specific type of ideas and images, which ultimately must determine the mode of life and career of the affected individual, especially in his formative years. However, the fact that about 25 per cent of the products of imagination can be determined by suggestion does not necessarily say that about one quarter of our rational life is also controlled by external influences. For it must be remembered that the social influences are multifarious and diverse in kind. Hence it is not at all unlikely that, like the myriads of chemicals that fill the ocean, they become neutralized in their effect upon us. This, however, will depend upon the state of civilization in the given community.

## THANATOPHOBIA AND IMMORTALITY

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By G. STANLEY HALL

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The psychology of death has very much in common with that of love, especially from the new genetic and psychoanalytic viewpoint. Each has an unenvisagable fact at its core as a point of departure, the one a putrefying corpse, the sight of which started Buddha on his career, the other the sex act; the one the most horrid, the other the most ecstatic of all human experiences. The inutterable psychalgia of the one and the shame and modesty that veil the other have in both cases made and used the same mechanisms, such as fetishisms, diversion, repression, over-determination, sublimation, etc., and by their action from each of these cores have evolved the most manifold and elaborated superstructures that have played a tremendous rôle in human culture. There is a sense in which all fears and phobias are at bottom fears of death or of the abatement or arrest of vitality, and also a sense in which all desires and wishes are for the gratification of love. The one is the great negation, and the other the supreme affirmation of the will to live. Orientation toward these two poles of experience is not innate in the sense of being finished and operative at first but is quite gradually acquired. The real meaning of death is not understood until puberty but both death and love show fragmentary and generally at first automatic outcrops from early infancy on. Freudians have shown how love has its artistic expressions in the forms of infantile reflexes, almost from the first, and we will first attempt to point out how far analogous prerepresentations occur in children with respect to death. Fear of death is only the obverse of the love of life and together they constitute the struggle to survive.

The development of the fear of death or *thanatophobia* in *children* is a striking case of recapitulation. The infant, like the animal, neither knows nor dreads death. The death-feigning instinct in animals is only cataplexy, and the horror of blood that some herbivora feel is not related to death.

From Scott's<sup>1</sup> 226 cases and my own<sup>2</sup> 299 returns to questionnaires, it appears that the first impression of death often comes from a sensation of coldness in touching the face or hands of the corpse of a relative, and the reaction is a nervous start at the contrast with the warmth which cuddling and hugging were wont to bring. The child's exquisite temperature sense feels a chill where it formerly found heat. Then comes the *immobility* of face and body, where it used to find prompt movements of response. There is no answering kiss, hug, pat or smile. In this respect sleep seems strange but its brother, death, is still more strange. Often the half-opened eyes are noticed with awe. The pallor, shroud, and especially coffin are often focussed on fetishistically, the latter being a strange bed. The friends are silent and tearful, and the infant who has been permitted such scenes often turns away, perhaps almost convulsively, to whomever holds it, as if in fright. The crying, however, seems more reflex than ideational. Older children of from two to five also are very prone to fixate the accessories of death, often remembering the corpse but nothing else of a dead member of their family. But our data suggest that funerals and burials are even more vividly and often remembered. Sometimes these scenes are the very earliest recollections of adults. The memory-pictures of these happenings may be preserved while their meaning and their mood have completely vanished, and but for the testimony of their elders children would not recall later who was the center of it all. Henceforth the dead is simply an absentee, and curious questions are asked as to where the departed has gone, when will he return, why cannot the child go to or with him. The infantile mind often makes strange mixtures of its own naïve constructions and adult answers. The distinction between psyche and soma, of which death is often the first teacher, is hard for the realistic minds of children to make. Told that Papa or Mamma rest or sleep in the ground, they ask why there, where it is so cold and dark, why they do not wake, what they eat, and who feeds them, impulsions that primitive burial customs often elaborately answer by preparing bodies for re-animation, leaving food,

<sup>1</sup> Am. Jour. of Psy., v. 8, p. 67 *et seq.*

<sup>2</sup> Study of Fears, Am. Jour. of Psy., v. 8, pp. 147-249. See also Street, A Genetic Study of Immortality, Ped. Sem., v. 6, pp. 167-213. I am also indebted to two yet unpublished doctors' theses by my students, W. T. Sanger: A Study of Senescence, and R. S. Ellis: The Attitude Toward Death and Types of Belief in Immortality. Both 1915.

and utensils, with the corpse, etc. When told of Heaven above children have strange crass fancies, perhaps that the body is shot up to Heaven, the grave dug open by angels, or the body passed down through the earth and then around up; the body generally gets out of the grave and goes up to Heaven by night, etc. As the idea of soul begins to be grasped, it is conceived as a tenuous replica of the body hovering about, sometimes seen, though rarely felt. It may be talked to or fancied as present though unseen. Children's dreams of the dead are vivid but rarely dreadful. In general the child thinks little or nothing but good of the dead, and the processes of idealization, aided by relatives, may almost reach the pitch of canonization. The memory of a dead parent barely recalled may become a power shaping the entire subsequent life of sentiment, as if all the instincts of ancestor worship could focus on an individual parent. Some adults maintain quiet sacred hours of ideal communion in thought with their departed loved ones, and their yearnings make a favorable soil for the ghost cult of spiritism. This component of our very complex attitude to dead friends is also the stratum which crops out in the holy communion sacrament of the ghost-dances of our American Indians, in which the souls of all the great dead of the tribe are supposed to come back and commune with their living descendants. Just in proportion as the dead are loved does death work its charm of sublimation and idealization, and just as a child of either sex has loved the parent of the other sex, will he or she idealize a chosen mate snatched away by death. Thus, too, one factor in the belief in immortality is love, that must conserve its object though deceased, this factor being quite distinct from the transcendental selfishness that would conserve one's own ego.

On the other hand, young children often seem rather to rejoice at than to fear death. The excitement of all its ceremonies is intensely impressive and new. Some children naïvely express the wish, after a funeral is over, that someone else would die. They play funeral, striving with each other who should assume the central rôle of the corpse, which they feign well. One four-year old tried to kill a younger mate and several abnormal children have actually done so, in order to enjoy again the excitement of the death, funeral and burial. A sweet young girl was found dancing on the fresh grave of her baby sister, chanting, "I am so glad she is dead and I am alive," suggesting, not the ancient days of famine when every death left more food for those who sur-



vived, so much as jealousy at the diversion of parental attention and care to the younger child. Neurotic children often play with unusual abandon, as if to compensate for the depression, when they have just left the room where parents, brothers or sisters have breathed their last. Small boys who lose their fathers say, "Now I will milk, cut wood, bring up coal" and sometimes they put on the father's hat or shoes, and in many ways assume his rôle, while little girls whose mothers die become more tender to their fathers and the other children, feeling themselves in some degree the surrogate of the mother. Just as children of tender age far more often fear the death of others they love than they do their own, so they vastly more often wish the death of others they hate than they feel any suicidal impulses. The death-wish, once clearly felt and realized in consciousness may, in neuropathic children, set up a prolonged and morbid corrective process to strangle it, and psychoanalysis has given us many cases where over-tenderness to a parent or relative, so insistent as to become troublesome was motivated by the impulse to atone for a vivid wish of death, which the child may have made toward the object of its anger. Only relatively late is the death-wish generally directed towards enemies and the ambivalent life-wish reserved for friends. Even in the most highly evolved emotional lives this is only a question of preponderance, for if our analysis is not mistaken, there never was a death, even of a lover, that did not bring some joy to the survivor, swallowed up though this component be in grief. Were this not so, comforters and consolations would be no resources. We strive to think our dear ones are happier and more at peace, console ourselves first with precious memories, and then ascribe superior powers of transcendental enjoyment to the dead. Conversely, no savage ever killed the bitterest foe of his tribe without elements of pity and efforts to atone to the soul of the victim or his friends by saying propitiatory words or performing placatory rites. Even Hell and devils never kill the soul and there are spots and spells of remission of torment so that surcease and nepenthe are not unknown even in the inferno.

When children are realizing at the most rapid rate what adulthood means, they often have very serious struggles with a more or less intermittent but at times overpowering sense of their own *Minderwertigkeit*, insufficiency, or incompleteness, in the sense of Janet and Adler. Tolstoi has given us a vivid characterization of this impulse in a record of his own boyhood. His tutor flogged him, and he reacted as the

only way in which he could "get even" by not merely the thought of suicide but the vivid imagination, well set in scene, of himself dead and his father dragging the terrified tutor before the beautiful corpse and accusing him of having been his murderer, while the friends around bemoaned him as so brilliant yet so tragically driven to death.

Nevertheless the love of life is in general strongest during these early pubescent years, and the death thought is chiefly a characteristic and flitting ambivalence or ebb in the flood tide of the will to live, for never is the momentum to be, do, know, feel everything possible, to maximize the ego, to obtain a pleroma of life and to touch it at every point, quite so great as during these years. Now life is, as Plato says, fastest. The will to live is extremely sensitive to external conditions, fluctuating with them. If we follow the Freudians here we can say that even adults in the prime of life are prone to let down their precautions against the commonest dangers when they meet any kind of adversity. The loss of a dollar, a sore finger, or the illness of a friend, we are told, makes us more liable to be run over by an auto or to let down our ordinary hygienic precautions, whereas the more life contains for us the more ideal our regimen. The case of the artist Segantini is in point. He worked with prodigious enthusiasm to attain his ends, finally lived alone in a hut on the Alps to catch their spirit on his canvas, but after being criticized he threw all precautions to the winds and went out recklessly in the night to commune with his muse, dying as a result.<sup>3</sup> In general as we advance in life the death fear becomes less acute and especially less spasmodic, and in the dying, on whom there is now quite a little literature, there is very rarely dread. In Osler's study of five hundred death scenes, he found most tranquil, and as the dermal senses became obtuse and pain ceased, the eye and ear were often very acute so that the last moments were in the closest kind of rapport with the environment. Often the last thought as the soul launches out to cross the bar is for others. There is often a tenacious clinging in thought or perhaps physically to a friend, and there is very rarely and almost never any concern for the individual's future, so that such death-bed scenes as the clergy used to paint a few generations ago do not occur. Thus death is very hard to conceive and interpretations of what it really means and is differ in every age and race, if not in every individual and almost at every moment of

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<sup>3</sup> Giovanni Segantini, von Karl Abraham, Leipzig, 1911, p. 65.

life. Death is primarily negative, privative, and, as nature abhors a vacuum, so the soul balks at the very idea of annihilation. Studies of senescence like Humphry's (*Old Age*, 1889, pp. 218) and Saundby's (*Old Age*, 1913, pp. 312) suggest, though they do not plainly teach, that those who have completely lived out every phase and stage of life to its uttermost and well on into the postcentenary stage without becoming senile or falling into dotage, begin to feel the ebb of the tide of the will to live, as if a counter-will to die was beginning to take its place. Thus if we do not feel cut off prematurely, with powers still undeveloped or capacities of enjoyment unused, we shall long for relief from life's fitful fever and fly to death as a welcome goal, a longed-for consummation. Stekel has most strongly urged that at bottom all fears if analyzed, are fears of death. The king of terrors may represent the supreme fear, and all ills and pains of all kinds and degrees may be interpreted as fears of partial death, because they abate vitality or check the momentum of the evolutionary struggle to survive that expresses itself in man's supreme desire for more and longer and larger life. But young children, as Ferrero<sup>4</sup> showed, like animals, never fear death *per se*, but only pain. Recent studies<sup>5</sup> of children's suicides show that although they begin at the very dawn of school age, they are augmented by all repressions of their natural interests and instincts. Only at puberty or after, when the life of the race begins to dominate that of the individual, do children commence to comprehend what death really means, and even then, as the 58 suicides of German school children per year from 1883 to 1905 show, many if not most are sudden, impulsive, and probably the majority, at least those of pubescent girls, are for the sake of the effect their death will have upon their nearest friends and relatives. What child has not seriously conceived suicide, at least in revery? Several partial censuses have been unable to find one.

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<sup>4</sup> La crainte de la morte. *Rev. Sci.*, 1895, p. 367 *et seq.*

<sup>5</sup> L. Proal: *L'éducation et le suicide des enfants*, Paris, 1907, p. 204. G. Budde: *Schülerselbstmorde*, Hannover, 1908, p. 59; E. Neter: *Der Selbstmord im kindlichen und jugendlichen Alter*, 1910, p. 28; L. Gurlitt: *Schülerselbstmorde*, n. d. p. 59; Baer: *Der Selbstmord im Kindesalter*, Leipzig, 1901, p. 85; Eickhoff, *Die Zunahme der Schülerselbstmorde an den höheren Schulen*; *Zts. f. d. evangel. Religionsunter. an höheren Lehranstalten*, 1909, v. 4; Eulenburg: *Schülerselbstmorde*, in *Der Saemann*, 1909, v. 5, p. 30; Gebhard; *Über die Schülerselbstmorde*, *Monatss. f. höhere Schulen*, 1909, v. 3 and 4, p. 24; Wehnert: *Schülerselbstmorde*, Hamburg, 1908, p. 81.

If death is the great mystery to adults, far more so is it to callow fledgling youth. So little is it understood by them that it is hard to utilize the fear of it for motivating hygienic regimen. To tell a child that Irving Fisher has found that by conforming to certain well established laws of health life may be prolonged on the average fifteen years seems a far cry, and is ineffective, for the child is absorbed in living out all the possibilities of the present reality. Moreover, there are other perhaps more obvious perils in turning on the death fear as a eutheic motive for children. During adolescence the death problem becomes a veritable muse, inspiring endless dreads, reveries and perhaps obsessions and complexes of the most manifold kinds, especially in neurotics, in which infantile impulses and adult insights are strangely mingled, producing weird perversions in later life. All these mazes one can never thread without a knowledge of the impression death has made upon the impressionable soul of man, all the way from infancy up through youth. Let us pause here to sample a few data from our returns from children supposedly normal.

Little children often *focus on some minute detail* and ever after remember, e. g., the bright, pretty handles or silver nails of the coffin, the plate, the cloth binding, black for adults or white for children, their own or others' articles of apparel, the shroud, the flowers, and wreaths on or near the coffin or thrown into the grave, the candles, the stray phrases of the preacher, the music, the incidents of the ride to the graveyard, the fear lest the bottom of the coffin should drop out or the straps with which it is lowered into the ground should slip or break, a stone in the first handful or shovelful of earth thrown upon the coffin, etc. The hearse is almost always prominent and many children want to ride in or on one. This conforms to the well known laws of erotic fetishism, by which the one item which alone can find room in the narrow field of consciousness is over-determined and exaggerated by the many items that are not able to get in. Most of the factors in thanatic fetishism are *bewusstseinsunfähig*. Children sometimes play they are dead, even when alone. They stretch out in bed, fold their hands, and hold their breath as long as they can to see how it feels to be dead. A few in fancy feel ill, imagine doctor and nurse, go through the last agony, and perhaps imagine others standing about weeping and praising them, or in other cases they personate the bystanders and see the imaginary death of a friend, and try to weep (though grief is hard and late to understand, and children often think tears a pretense). They often pick out pretty coffins for

their chums and even imagine becoming burial frocks. The odor of varnish from the coffin sometimes has an almost incredible persistence and power to call up feelings and emotions. Many children fear that the corpse will wake and sit up. "He is not dead but sleepeth." Many are the reports of how by calling, touching, pounding and otherwise doing forbidden or commendable things, children strive to provoke or coax their dead relatives to awake. To them, too, death has many degrees. The buried body is deadest. It is more so in the coffin than before being placed there. A very sick person who may die begins to be invested with the same awe. Lying in bed by day, the doctor, the silent nurse, the smell of medicine, often suggest that death has begun. Toward very old people children feel something of the same awe because they must soon die. A very few are incipient nekrophiles, trying to stroke, handle, and even kiss and hug the corpse. Scott's curves indicate that at about the age of five death is more likely to seem attractive and interesting, while at about nine its real horror first begins to be actually felt. Some at a very tender age acquire associations that persist for years and occasionally through life with specific incidents. In our data adults mention, e. g., the sight and smell of tuberose, any black box, even a black boat, a crepe veil or bow on a doorhandle or even a hat or garment, the tolling of bells or even the ringing of them, phrases in song or scripture as in family prayers, while in one case every allusion to death had to be omitted to prevent an hysterical outbreak in a young girl. Some go far around to avoid passing an undertaker's shop, etc. One young man felt a sudden horror toward a young lady to whom he was attached, as soon as he learned that she was employed in an undertaker's shop. The suggestion of these things may cause a sudden convulsive sob, tears or inexplicable depression. Some report acute panics when gazing at quietly sleeping friends, lest they may be dead or dying. Children's funerals, interments, graveyards for pets, are now represented by a small literature. How they sometimes love to play with death-shudders in their talk and thoughts is well illustrated in the case of two girls of seven whom I once overheard while watching a man on a very high roof. One said, "Oh, I wish he would fall right down backwards and kill himself." "And they pick him up all bloody," giggled the other. "His bones all broke," said the first. "And put him in a black box in the grave," said the second. "And all his children cry," said number one. "And starve to death," added the other. They were getting more excited,

awed and spoke lower as they passed out of my hearing. The horror, and yet sometimes the fascination, of a room where a friend has died or a place where a murder has been committed is well known.

The "*death-thought*" in some of our data seems almost spontaneous. It breaks out obsessively or on the slightest occasion. Some have spells of crying with wild abandon at the thought that they must die, which sometimes seems to sound out to them as if from the welkin. It is worst nights. It seems so unspeakably dreadful that they cannot steady their voices. One clergyman was so haunted by it that he could not conduct funerals, and only after years was he able to find self-control in the conviction that he might live on until Christ's second coming and so not taste death. The thought in the infant prayer, "If I should die before I wake" made one child more or less neurotic for years with the horror of Hell and judgment, and she was wont to fancy herself found dead in the morning, and used to pose for it to look her best. Several grew hysterical at revivals. Some who have been very near death by drowning or other accident recalled that their minds were entirely pervaded by such trivial thoughts as wondering who would do this or that duty they had performed, realizing that they would escape something disliked or that some enemy would be glad, that now they were going to find out all about it, etc. Thereafter this memory of the painlessness and pettiness of their experience in *articulo mortis* robbed them of all further fear of death, and yet subsequent recall of the scene after narrow escapes may sometimes produce the very opposite effect and bring access of fear and dread. But one characteristic of later childhood and youth, and sometimes maturity and old age, is that in some quiet hour, perhaps in a wakeful moment in the middle of the night, or more when alone on the shore or in the forest, the thought, "I must die," seems to spring and fasten upon the soul like a beast of prey. It flashes out with great and absorbing vividness. In a few cases a voice seems to pronounce the sentence. Occasionally this is so intense that the child fancies that it is in the act of dying and springs up in terror. Morbid fears of death always are regressive or reversionary with childish features.<sup>6</sup>

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<sup>6</sup> Mersey (La Tanatophilie dans la famille des Hapsbourg, Rev. d. Psychiatr. Nr. 12, 1912, p. 493) describes the strange case of love of death in the daughter of Ferdinand and Isabella and also Charles V.

What are the outcrops in early childhood of the effects of the fears of death in the race, for the child usually begins, like the animals, with no fear of it, and epitomizes in the successive stages of its individual existence all the steps taken by the phylum. We certainly do see buds of about all the

The former, after the death of her husband, Philip the Beautiful, whom she loved with a consuming jealousy, had his body embalmed and only with great difficulty could she leave the coffin where it lay. Sometimes she had it open for a time to kiss the bare corpse, and did so with the greatest passion. This state had periods of remission and exacerbation. The history of Charles, too, can be paralleled in many modern instances, while dreams show us still more clearly how nekrophilic man can be.

Witry says that from his own practice he believes thanatophobiacs are almost always from the professional or upper middle classes, those from the lower classes meeting death with more stoicism than those of the upper. Catholics, he says, have little fear of death. Thanatophobes are usually neuropaths of degenerate heredity. One of his cases, a girl of 18, was suddenly seized by a violent fear that she was to die within an hour. She was put to sleep by suggestion and woke up normal. A woman teacher of 49 had three acute attacks, cured by suggestion. A middle-aged physician, after being drunk, had acute fear of death and Hell, which yielded to medical treatment. Old priests, we are told, are especially subject to it if neuropathic or "*scrupuleux*." Some feel it acutely when, after fighting a long reluctance to do so, they have compelled themselves to make a will.

Ferrari (*La peur de la mort*, Rev. Scient., 1896, v. 5, p. 59) describes several cases of tolerably healthy people who have had sudden premonitions of death, with acute fear, and who have shortly thereafter died, some of them from no ascertainable cause. Hence he raises the question whether an obsession of death can be so strong as to cause it.

Fiessinger gives a case which he thinks directly due to the symptoms of *angina pectoris*, and discusses whether patients should be told their disease and its gravity, in view of this possible phobia.

Ferrero (*La crainte de la morte*, Rev. Scient., 1895, v. 3, p. 361) thinks the natural man has little fear or thought of death and its representations in art and religion are not painful, on account of the sustaining influences of our organic sensations. Still, the thought of death does have much influence upon our ideas, and to some extent our sentiments. The mathematical chances of death play a small rôle in affecting the choice of professions. It is only the prospect of impending death that shocks. Chronic invalids have little fear but only hope for life, *e. g.*, consumptives, while to some, *e. g.*, Indian widows, lovers, it is attractive. Hence he thinks it normally indifferent and sometimes agreeable but becomes an object of fear only by association.

Wilson (*The sense of danger and fear of death*, Monist, 1902, v. 13, pp. 352-69) thinks all creatures may be divided into two classes, each with its own characteristic reaction to stimuli, (a) the predatory or pursuing, and (b) the fleeing, and as each experience leaves an organic legacy which atrophies very slowly and that in the subconscious, these differences are intensified. Fear is an evil which can be eradicated only by concentrating on work and not on self.

Levy (*Die agoraphobie*. Wien. allg. medicin. Zeitung, 1911, nr. 10)

characteristics of the many and diverse attitudes which the race has assumed toward death, but this is not the place to trace these in detail. Moreover, such reactions as the above in consciousness are only the epiphenomena of the larger, deeper, evolving processional of the unconscious and even organic responses to it, and each of these needs analysis from the patent to the latent before we can find what the soul really means from what it says. We can, however, make the provisional answer to the above question, and that with considerable confidence, that death is not unlike sex in that while the components of death-attitudes are early present, these elements are not much organized into unity until about the time that the *vita sexualis* as such develops at puberty, when racial experience in both fields comes to more or less conscious unification. The normal child is dominated by the basal impulse to live and grow, and there is little room or possibility for the realization of death. Death has no business with the child or the child with it, any more than is the case with the interests of posterity during the age chiefly devoted to individuation. Life and growth of psyche and soma are at their flood tide and every intimation of death is not only foreign to the very nature and needs of the child but is arrestive of the course of nature and should be, so far as possible, veiled in reticence, like sex, with only provisional answers to the genuine questions about it. The above data show how completely the *genetic impulse shields the child* by diverting it from the central fact to countless irrelevancies, trivialities and accessories. Just as the instinct of the race has blindly striven to avoid sex precocity, if not to delay puberty, and more consciously and purposively to enforce a period of repression between the age of pubescence and that of nubility, so myth, primitive religions and even Christianity have provided many ways of mitigating, even for adults, but more especially for the young, the nameless horror of direct envisagement of the fact that we all must die and cease to be body and soul, or, like the Nirvana cult, to make it all tolerable. This cult is one of the boldest and most advanced if not the truest and most effective of all the anti-fear death cults.

At the opposite extreme stands *Christianity*, the culminating achievement of which took place in the few days between

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gives a case of an agoraphobia which was rooted in a very distinct dread of death by a special disease. A Dubois psychotherapeutic conversation which proved the fallacy of its grounds and to which the patient attended, although with great effort, did not quiet but only increased excitement. Excitement and exhaustion were the chief symptoms and the case yielded only to isolation and rest.



the burial of Jesus and the Pentecostal outburst. Never in history, if it be history, and never in the subjective story of Mansoul, if this be the stage on which it was all accomplished, has there been such *au rebours* from the nadir of depression of the disciples, because the type-man of their race, who had grown to their minds to be a fully diplomated God-man, was completely dead and that in shame and ignominy, and his corpse sealed up to moulder and rot in a rock. Then came first the timid and then the plenary conviction that He had conquered death and even Hell, risen from the dead, walked and conversed with friends in an attenuated body and visibly ascended to Heaven and God. Once fully convinced that this was all veritably true, witnessed and attested by every sense and proof, the great incubus of ages was removed, and death, the supreme terror, was abolished. This brought a frenzy or saturnalia of joy called the gift of the Holy Spirit, which possessed their lives. The ecstatic disciples shouted in weird unknown tongues, until onlookers called them drunk as with new wine, gazed all day into Heaven, henceforth the home of souls, and had to be exhorted to cease their raving jubilations and go to work. In this inebriating new joy and freedom they, and later their successors, met the nine persecutions, during which martyrdom became a passion, and tender youths and maidens could hardly be restrained from throwing themselves to the wild beasts in the arena as the supreme crown and testimony to their faith. So, too, Christian asceticism followed from the same motive. This life was mean and it mattered little how squalid it was, for it was only a provisional, probationary moment compared to the eternal joy and happiness where all real worths and values were confidently awaited, compared to which those of earth were only dross. "There is no death. What seems so is transition" to an infinitely higher state than this. Never did the other world so absorb the power of this. Visions, trances, homilies, poems, poetry and theology fitted the other world out with every good, and the chief offices of the church were to keep the keys of the transcendental world and to wield its tremendous sanctions in a way to dominate life and determine good and evil. Thus never was the greatest *Verdrängung* that ever oppressed the human race so completely removed. The most essential claim of Christianity is to have removed the fear of death and made the king of terrors into a good friend and boon companion by this the most masterly of all psychotherapies. If it be only a pragmatic postulate or hypothesis or *Als Ob* in Vaihinger's sense, it has worked well on the whole, despite

the ever present dangers of transcendental selfishness that prompts only to save one's own soul, nevertheless it is the supreme demonstration of the *Allmacht* of the folk-soul to minister to its own gravest diseases and banish its greatest enemy, the death fear.

In all we know of the folk-soul there is no more striking illustration of geneticism than the slow but sure establishment in recent years, by comparing ancient myths and rites with the findings of excavations, that in the great countries about the eastern Mediterranean, especially Thrace, Asia Minor and Egypt, the highest religious consciousness of these races was expressed in elaborate cults of death and resurrection, to have participated in which is said to have made the celebrants over and initiated them into a new and higher life. All was so secret and oath-bound that it found little representation, save the most incidental allusions in history and literature, so that it was reserved for modern research to uncover, reconstruct and understand its tremendous power. Osiris, Persephone, Attis, the lover of the all-mother Cybele, Demeter and Dionysius in the Eleusinian mysteries, Astar in her restoration of Phanaeus and many others, some with very high and full and some with very scanty and fragmentary developments of the myth and cult, died and perhaps went to Hades and came back bringing, now one, now many with them. Typical of these ceremonies were the funereal sadness, death dirge, wailings, active symbolic manifestations of grief and despair, as if to attain the very acme of psychalgia. The great, good, beautiful, divine hero is not only dead but has perhaps gone over into the netherworld to defy death and the power of evil in their stronghold and to conquer and bind them. There is, then, a phase of painful, anxious, silent suspense. Will he succeed and return, or will he fail and never reappear? Then, when the tension is at the very breaking-point, comes the thumic ebb, rebound or reversal. Someone whispers or cries aloud, "He has won and comes back," and then all is changed. Lights flare out in the darkness. Instead of tears and sobs there are joy unrestrained, congratulations, embraces, and soon frantic ecstasy, leaping, shouting, wine, song, revelry, bells, cannon, fireworks, and sometimes in degenerate days drunkenness and gluttony with the sacramental elements, and in token of the triumphs of the higher love, carnal debauch and revelry, and always ecstasy and inebriation with euphoria. Thus from three to six centuries B. C. men strove to attain an immunity-bath which should safeguard them from all excessive pain and pleasure

of life by participation in a pageantry or dramatization of the eternal struggle between the greatest evil, death and the dread of it, and the greatest joy of the most intense living, thus ensuring their souls against being led captive by pleasure or by pain, by keeping wide open the way from the extremest depression to the maximum of exaltation.

Now all this rests in every case where it can be traced, upon the *retreat of the sun and the death of winter* in autumn and the return of spring, re-enforced of course by the alternations of day and night. These deities or their prototypes were originally gods of vegetation and the resurrections are vernal. The everlasting bars that broke were snow and ice. The king of glory that came in when the gates were lifted was spring, the conqueror, and in these secular changes of the year are found the first preformations of the soul and the momentum that still subconsciously re-enforces that belief in a life after death and supplies always an anodyne and often an antidote for the death fear.

Our thoughts of the fate of the bodies of our friends do not stop with interment, but we have little to guide them save the traditional poetic abominations of putrescence, worms and mouldering to dust, which are as unscientific as are the amorist's conceptions of the heart that breaks, swells, rises, sinks, sings, and is and does other physiologically impossible things permissible only to poetic license. I cannot find a single study of the chemical changes that take place through subsequent weeks, months and years until only skull and bones remain, although Finot (*La philosophie de la longévité*, Paris, 1900) says our body is immortal in the larvae born in our decomposed flesh. He describes the succession of the fauna that consume corpses and sees in the very maggots and vermin that horrify men (a phobia to which Tolstoi has given most acute attention) sources of consolation. Thus the great rhythm goes on, birth and death, but the forms of life in the grave from which evolves life may proceed on and up to higher forms. The so-called peace of the grave is really full of vitality. The thanatophobia of the race and the repulsiveness of carrion have kept the doors of the tomb effectually locked, so that we know far more of excrement than we do of decaying bodies of men. One of the most universal instincts of the human race is that the dead must be put out of sight else, at least for the Greeks, the ghost cannot rest but haunts those who have neglected this primeval duty. The conscious *morituri* crave decent burial and to provide this is the most imperative duty felt by survivors, and from burial on friends

always strive to think of the body as it was and forget what it is. Even tombs or gravestones have this diversion from reality as one of their purposes, and the reveries that play about the content of the casket beneath, active as they often are, especially in the young, are sternly suppressed, even though they burrow beneath the psychic humus and reinforce unconsciously death-thoughts and -phobias. Children have a stage of interest and sometimes more or less absorption in dreameries as to what is going on down there where the buried friend was laid to rest, but convention checks this almost as severely as it does their naïve queries concerning sex. Now it is this suppressed mentation that has contributed not a little to motivate the various modes of burial such as desiccation in trees, mummification, sealing up from the air in urns, rocks, etc., cremation, water-burial of various kinds, interment under cairns to protect from beasts, or exposure to birds of prey, as in the Persian towers of silence, or eating relatives or foes ritually, whole or in part, as is done among some cannibals, devising for the use of anatomists or medical schools, special treatment for certain parts, or bequeathing them to special uses (e. g. the skull for a drinking-cup, skin for drum heads, joints for glue, intestines for bass viol strings, pericardium for money-purse, etc. as we occasionally find among fanatics). Only very recently have hygienic considerations begun to come to the fore, for it is hard to realize that the dead body of the dearest friend may become a source of death to those that loved it while living. The reluctance of those contemplating death or their friends to submit to post-mortem examinations, even for the benefit of science, is a relic of nekrophilism that once prompted embalmmnt and still lavishes wealth on gorgeous monuments because the blank fact of death is so hard and repulsive to grasp. I am convinced that an analysis of burial customs makes it plain that many if not most modes of disposing of the dead (such as Yarrow describes, volume 1, Bureau of Ethnology Reports) are motivated in no small part by the impulse to repress or divert from thoughts of putrescence, and that the belief in reanimation and another life, though often evident, is far less prominent than most anthropologists, not to say all theologians, have been wont to assume. A body embalmed, burned or hermetically sealed, cannot be thought of as putrid or spreading infection. If some modes of burial would make physical reanimation easier by keeping at least the more important nuclear parts of the body together or intact, others quite as common would make this far harder. One factor

that must never be left out of all these explanations of forms of disposing of the dead is thus to prevent the survivors from dwelling upon the natural processes of disintegration, so intolerable that all fashions of burial are primarily to conceal it or to divert the mind from it. If a Freudian term be permissible, they are so many *Deckphenomena* which the folk-soul have devised to cover up one of the most insistent but repugnant thoughts, viz., the decomposition of the bodies of our dear ones. How can, for instance, a lover who to-day dotes and gloats upon the eyes, mouth, and every part of the body of his inamorata, next day contemplate her corpse destined to rot through a series of stages, from every one of which every sense would turn away with horror? To mitigate such a shock and to save the psyche from disintegrating under it all these vicariating, easing and defensive mechanisms have been slowly evolved. Their worth is in what they save us from, more than in what they give. They are to protect us from obsessive, imperative thoughts of the body's decay, rather than to assure us that an undying part survives and will again want its sarcous tenement. They are, rightly conceived, therapeutic systems to assuage the thanatophobia that might drive at least neurotics mad with fear and dread. This obsession, then, traditional sentiment and even the belief in a post-mortem life of the soul, helps us to relieve.

The most romantic effort to near, face to face, envisagement of physical and chemical facts is the vague sentiment not infrequently found of late in poetry where consolation is sought in the possibility that our bodies when resolved into their elements may, if ever so little, affect the phenomena and processes of Nature. "Great Caesar's body, dead and turned to clay, may stop a chink to keep the flaws away." When in a cremation furnace we see a coffin melting away and the body going up in beautiful polychromatic flames, two or three thousand degrees Fahrenheit, an imaginative but scientific friend suggested that the spectroscopic lines from these flames might some day tell us with unerring certainty and great detail whether the dead had led a good or a bad life. Something of our soma may at least make grass and trees grow faster or flowers bloom more beautifully, or add some faint tint to the setting sun as the body becomes a diffusive power, further and further irradiating out through the universe. A Hindu ascetic changed his mode of life and eating and underwent a dietetic conversion in order that the "dear brother worms" might have a happier time and a more sumptuous meal from his body when he died, for would not

his decaying carcass be for a time heaven to them, and when they were fat they might in their turn give a rich repast to the beautiful singing birds or feed their starving young, and when the birds in turn grew to be fowl, they might feed man and give man more strength to serve God and be a greater blessing to his fellowmen. Perhaps, too, there are laws governing disintegration of the molecules that compose us, that would, were they really known, teach us very much of evolution by setting forth all the converse stages of physical devolution, or show how the life of disease germs is affected by the death of their host, etc. But all this is so far a wain, falsetto thought, even though it may be a symptom of convalescence from a great fear. Yet who shall say that it may not point out the direction of research that, when man is thoroughly cured of his death phobia, science may take, and find a field that may yield rich results not only for knowledge but for courage. As yet the age-long cowardice is too little overcome to permit us to strike out boldly into it, for the race still shudders under the spell of this phobia as of no other.

*Epitomizing very roughly*, we may say (1) that fear of corpses is quite primitive, although found in infants only in the form of a nervous shudder set off by coldness, immobility, pallor, sunken and partly closed eyes, etc., on a background of strange surroundings with the social contagion of grief. The death-fears of generations of human forbears are inherited in the form of neural predispositions to shudder, but with no intelligence and little or no consciousness. This is more reflex than instinctive but in it is the promise and potency of a mass of later and higher reactions. Here the analogy with sex is close for *ludeln* and *lütschen* and infantile anal and other isolated prelusions of sex are on the same level. They are fragmentary partial *Triebe* with no awareness of sex but are only the first ontogenetic expressions of a long phyletic experience with sex. As the first naïve infantile curiosity about sex is soon powerfully repressed, so the first interest in death suffers multifarious *Verdrängungen*. So effectively have shame and modesty stamped themselves upon our organism that we have many cases in which the first envisagements of sex cause a painful and normal repulsion, as in the case of the servant girl who laid an infant on the floor to make the bed, and by stepping over it aroused in the babe an aversion for all girls that lasted well on to puberty, or as in Ernest Poole's "The Harbor" the boy of

seven saw on a forbidden street a gross scene between a drunken sailor and a vile woman, which killed for a long time the great previous charm of that general locality, and again in the early teens when he saw the red garter that had accidentally slipped down over the ankle of his favorite girl playmate of twelve, he felt an aversion that sent him suddenly away from her for years, unconscious as it all was on the part of both. Such cases illustrate how effectively nature arms the normal soul against sex precocity. In the same way she would shield the young child from the premature realization of death and allow it to linger in the animal plane before it was known. That nekrophilism has its germs in infantile experience as truly as does anal eroticism there can be little doubt, although we have as yet few analyzed data on the subject. It of course goes with the fact that death is not known for what it really is.

(2) The instinct to deck out, to beautify and perpetuate the corpse, occasionally to photograph and in Egypt to mummify it, is less analogous to the impulse to purify the exhibition of the nude body in art or to sacramentalize the sex act in phallic cults. Both tendencies developed in the teeth of a strong contravalent trend. Erotism toward corpses in the sense of Krafft-Ebing and Tarnowski might stand for the extremest form of the negation of death just as a truly immaculate conception, with suppression of all passionate lust in the interests of the offspring, would be the ambivalent pole, while mummification would be more like the ritualization of coitus; but in an article like this one can hardly dwell upon the details which to my mind validate this thesis.

(3) Diversion is plainer. From the fig leaf or breech cloth up to the wedding dress, flowers, gifts, nuptial ceremonies and customs, we have *Verschiebung* from the acts and organs in which wedlock is consummated to hair, face, arms, movements, ritual, music, dot, honeymoon, etc., in ever widening irradiation. So in death and funerals, attention is directed to grave clothes, the coffin and its trimmings, flowers, perhaps incense, chants, solemn religious ceremonials, feasts before and after, wakes, vigils, ending in a formal commitment of the body to the natural forces of disintegration. Of the bride and groom, as of the dead, we speak only good, and we would almost as soon speak ill of one lover to another as of the dead to surviving friends and mourners. As death of one mate often revives love where it had begun to languish so separation may fan the flame of affection and make for

idealization. Death beautifies and perhaps beatifies, and also awakens the conscience of the survivor at the memory of real or fancied mistreatment.

(4) One of the chief causes that first suggested and then made man cling with such persistence to the belief in souls was the far greater difficulty in grasping death as annihilation. The passing of the body cannot mean the end of all. Something must survive for the mind like nature abhors a vacuum, and hence we have to postulate something in place of the vanished body. The dead thus are not quite dead and from this faint suggestion slowly evolved the primitive ghost cults and finally fully panoplied heavens and hells, with the conception that the body was a mere husk or shell which, when it was sloughed off, liberated a far more glorious and enduring soul. Thus belief in the immortality of the soul arose partly as a compensation which man's autistic nature evolved to make up for the realization of the mortality of the body. It was a consolation-prize, precious because it atoned for the supremest of all calamities. So love, at first purely animal and selfish, slowly came to realize that it was not a finality but that it was for the sake of offspring. Love that is only physical and personal satisfaction means for fallen man the gradual extinction of his stirp, while eugenics, which is a new religion of life, regards chiefly the immortality of the germ plasm. It compensates for a love that is only scortatory, supplying another object than the mate, viz., the child, in which not only does passion find a vicariate but which gives an object on which those unmated can lavish all their affection. Especially parents live on in their well-begotten and well-bred children, and when senescence has sapped the roots of mere amorousness, love for offspring is distinctly reinforced, as we often see in the assiduity and indulgence of grandparents, which so exceed that of parents. Thus love provides for itself in the aged and in the unwed a normative surrogate for posterity. We have been told that the most prolific races and social classes in the Occident are those that believe in the immortality of the individual soul, and conversely that the least so are those that doubt it. If this be true, we have yet to find the mechanism of this correlation, but the fact, if it be such, is of the utmost psychogenetic as well as pragmatic importance.

(5) Another great product of the fear of death and its agent, disease, is medicine and hygiene, to say nothing of all forms of life and health insurance, etc. Man's deepest impulse is to live as long, intensively and richly as possible,



that is, to attain macrobiotism. Everything that checks this maximum is lethal, for both life and death are of all degrees, death being only the zero on the life scale. Fear is not merely the will to be immortal as Stekel (*Nervöse Angstzustände*, zweite Aufgabe, 1912) defines it, but the will to live out completely all that is in us. We realize every possibility and expand every dimension of our nature. We long to be just as well, strong, happy and vital as possible, and strive against everything that impedes this wish or will, while we imprecate even our parents if through their fault we are born short or handicapped with disease. We love life supremely and cannot have too much of it, and foods, drugs, and regimen, are precious just so far as they minister to this end, while we dread all that interferes with it. This lust for extreme individuation, however, has at once its consummation and its reversal in love. It is at once the acme of self-affirmation and of self-renunciation, for it is no less kenotic or self-emptying, since subordination also begins in love, which must serve. Just as what we know as anger consists of only partial and almost erratic outcrops of the generic aggressiveness of man by which he has conquered nature and his enemies, and has explored, investigated, and accomplished all his active achievements in the world, so what we see as fears and phobias are only the remnants and almost random residua of man's passivity and plasticity, which have from the beginning made him receptive and docile, which have culminated in the development of memory and science, and which began in adjustments to the ways of nature and of the social environment. This change is one of the most pregnant of all the insights of genetic psychology, and shows us how knowledge is itself so largely a product of fears (which culminate in the fear of death) and their correlate, the love of life. Each of them has innumerable degrees and of these art and religion, no less than hygiene and science, are the results, for all of them are but progressive realizations of the ever more patent possibilities of human nature with all its still slumbering and yet to be evolved latencies. Of this general thesis let us now consider some of its items more in detail. We must first, however, consider the *genesis of ghosts* and why we fear them.

The all encompassing atmosphere, near the bottom of the deep sea of which man spends his life crawling about, is the chief psychogenetic basis of his conceptions of things immaterial. We sense air in motion by touch, the mother of all the senses, and hence its reality is as certain as this, the most

realistic of all the senses, can make it. As wind it may sweep the sea and land and leave wreckage in its wake. Its power may rise in crescendo to a degree which trees, houses and even earth and stones cannot resist; yet terrific as its dynamism may be, it is invisible and so it is invested with a certain vague and mystic awe. It parches and brings rain, propels clouds and bears the lightning. It can lift, break, transport things or buffet us on all sides and yet it is bafflingly unseen. It lashes the sea, brings heat and cold, and wet and dry, controls flood and withering drouth, makes climate, brings health and disease and yet is without color, shape or outline. Its direction and intensity have had many personifications. It is the medium of communication with the gods. As breath it is life and its absence is death. It was once and long regarded as the third potentialization of the four elements or as earth and water sublimated and liable to pass anywhere over into the highest of the four, fire. Arteries, as the name signifies, were regarded as air passages in the body until Harvey appeared. Inspiration was caused by inhalation, especially of the higher, more empyrean air on the tops of sacred mountains, for this the gods breathed. Most terms in most languages, such as *pneuma*, spirit, *nous*, *anima*, soul, ghost, etc., mean etymologically air. To air we thus owe most of our conceptions not only of psychic but of metaphysical reality. Air even at rest is closer to us than anything else objective, is constantly all about and in us, yet we cannot taste, smell, touch, hear or see it, though it conditions all life. It carries all voices, sounds and noises. To it thus man owes the psychic mechanism which created and sustained everything transcendent. From these unique sense data thought took its first and chief step in emancipating itself from things and thus were laid the foundations of the entire super-sensible world. The significance of this step is vast and not yet adequately realized. It marked a great epoch in human culture, though it was by no means suddenly but very gradually achieved. Thus, to primitive thought, all space was not void but filled with powers which were always appealing to us but to which we were hopelessly and almost totally blind. Here was the first great kingdom of faith as freed from sight, into which spiritual creations have always been projected. In light and dark alike air moving or moved against us is palpable and actual. Thus the greatest of all the playgrounds for the imagination was opened and furnished by fancy, which could escape the coercion of facts and revel in creations all its own. Wind deities so often first and

foremost in primitive religion and myth are made not only in but of air almost as truly as all life forms are made of protoplasm. God and ghosts in their nature and attributes are airy. The air is their home whether they float about or are organized into heavens, hells or limbos which are neither or both. They do airy things. They whisper, breathe upon or gently incline or resist us, although they may roar at, compel or overwhelm us. They move freely, fast and slowly, high or low, come unawares as zephyrs or suddenly as gusts. They beat and smite us, pipe softly or howl and roar. Thus air and its phenomena need but a touch of fancy to take on more or less definite personal forms. Now, had man crept about under the water near the ocean bottom, all his transcendentalizing mechanizing would have been far more crass and crude and had he been a mole-like burrower under the ground, everything metaphysical would have been impossible. Had man been viable in a world without air, he might conceivably have thought logically along the prime categories of time, space, and matter, but to air and its forms and forces, he owes about all his conceptions of bodiless individualities. Under the proclivities developed from such incitements his soul created the denizens of this realm.

Of these the most definite were ghosts of at least two kinds, those that are products of fear and pain on the one hand and those that express groups of wishes on the other. Those airy spiritual parts that survive, whether in animals or men, who were unfriendly to us or whom we have injured, persist and return to harm us. These we dread. Had we done to or experienced from these beings no harm at all, all ghosts would have been benign. Because we have been or deserve to be injured by them, we fear them. Had we suffered or merited no pain from them, their wraiths or sprites would have been at least indifferent and never hostile to us. Ghosts do not change their disposition in sloughing off the body and need placation by us just as much as or more than when they were incarnate. Hence, the dread of them is the product of either old fears or an evil conscience. On the other hand, kindly spirits wish us well for they are products of our wishes that our friends not only survive but minister to us and protect and warn us against evils. There is a psychic momentum of life by which we find it difficult to realize that those we knew daily and love dearly are really no more, especially if we have not actually seen them dead and buried. Funerals have always been the greatest ghost layers. Moreover, the psyche, like nature, abhors a vacuum,

and the dead persist not only in memory but in fancy. Both together contribute to the post-existence and influence of the dead. On this basis, when audition and even visualization and mental imagery are vivid, their creations easily reach illusory intensity and when to this is added the automatism of movement, speech and intensive idealization waking or especially sleeping, the readiest and most inveterate interpretation is in the sense of spiritism in one or more of its forms, and unusual occurrences in our bodies or souls or in those of others seem mediumistic. Disease especially is believed to be demonopathic and due to possession and minor abnormalities which may be organized into a secondary personality are interpreted supernaturally as inspiration from supernal powers. This makes all mediumnopathy an object of awe and respect. Thus all the powers of the air, potent but unseen, are developed and spirits strive to possess our bodies and souls. The ghosts of ancestors watch over us as in China. In the ghost dance, the red man communes with and yields to the influence of all who have preceded him to the Happy Hunting Ground. Ghosts wake and walk on Hallowe'en and we earn the good will and intercession of the departed on All Saints' Day. Back of all is the struggle of the individual to survive the last and greatest calamity, death, and this makes us all keen to believe and slow to reject every sort of proof. Thus the other world came to its immense power over human life, outweighs it and makes it often seem mean and contemptible. In the aerial kingdom life is not only perpetuated, but is idealized, is eternal, blissful, pure, so that all the heavens are asylums of unfulfilled wishes where every lack and want are supplied or else where all evil is; a realm of complemental and compensatory realization where we enjoy all which is lacking here or if we had escaped mundane justice, where we receive penalty; a realm to which the hierarchic orders hold the keys and thus become supreme over all other powers or at least a rival of kings. It has been the world's great resource where all who seek refuge find it. It is the great superstition that stands over us, the extra- or the *Aberglaube* as true to the heart of the race as science is to its reason, concerning which we may believe what we list or will. It has made heroes and fanatics. It has been the world's greatest bulwark against crime and vice wherever its sanction has been on the side of morality. To doubt it is the quintessence of all heresy and skepticism, and to have faith in its existence in and of itself has a saving efficacy. It has made theology out of anthropology by projecting the latter

upon the skies. Its formularies have been sacred. Faith, which is a vague combination of wishes and fears, is its organ. It is the world's greatest system of psychotherapy, both moral and physical, though had we been completely normal physically, mentally and morally it would never have existed at all, because unnecessary. As it is, Heaven and its contents and denizens are the truest of all true things. It is a sublime and intricate system of slowly accumulated projections into the *Jenseits* of pragmatic sanctions, but when the complete superman arrives, it will fade because its regenerative agency will have been accomplished. It is now the light and hope of the world, but when the day of immanence comes, that of transcendence will be done away.

*Why do men fear ghosts and feel uncanny about the wraiths of even their dearest friends, whom it might seem they would long to meet under any circumstances?* This question has often been asked but never answered. I think we may set down at least the following as explanatory factors.

(1) Ghosts suggest corpses and perhaps modes of death, and so are repellant by associative memory. They often wear grave clothes and perhaps preserve coffin attitudes, show cadaveric and skeletal traits in the head and its features and in the body generally, sunken eyes, cheeks, pallor, etc. They prefer graveyards, death rooms, and other localities associated with death. They may have scars or gaping wounds, may utter death moans or shrieks, and many of their characteristic parts and acts suggest lethal thanatic attributes. They may even have a charnel-house odor, and often diffuse a chill as a kind of aura. They rarely appear in full daylight but ally themselves to the instinctive timidity we all tend to feel at darkness.

(2) They are so ethereal, evasive of our senses, independent of gravity, able to pass through solid doors, walls, etc., like x-rays; are thought always to have much power over us and perhaps to have us at their mercy; no weapons or shelter or perhaps even magic formula can be absolutely trusted to keep them off. They are popularly thought to do uncanny things, to materialize, hover, exhibit or cause levitation; to rap, sound instruments and make other noises; to haunt places where the dead lie or abandoned houses. Thus the physical properties ascribed to them are mysterious and awe-inspiring, and we cannot understand their will or their ways. Though they may not be as dangerous as electric fire-balls, they are as weird, and as unlike a normal psychophysic personality as a fire-ball

is unlike a football. Even if they had no association with the dead as above, they would in and of themselves inspire a certain dread because of their incorporeality, and because they are a unique and incomprehensible aggregate of powers and qualities dominated by a personality.

(3) Implicit trust in our friends is rare in life, and there is often a stratum of suspicion toward even those we love. To win in the struggle for survival, we cannot commit our lives, fortunes, and sacred honor with no reserves or safeguards into any other's keeping. We often treat our friends as if they might become our enemies, and so have reservations even toward our friends and lovers. Now the dead are not restrained. They can work their will on us with all the power of the other world, and can act undeterred by any fear of consequences. They can vent their rancor and feed fat every secret grudge. They can merely tweak us like elves or pursue us with maledictions and scourges like furies. So we placate, seek atonement, make oblations. There are special dangers from those we have wronged or who die hating us, if they are unappeased.

(4) Ghosts very often come back to enforce justice, as they conceive it. They punish, seek redress, make restitution for neglected or violated duties, or perhaps become ministers of vengeance. When our friends die we are prone to think of many things we might have done or left undone to or concerning them. We are remorseful if we have their anger, penitent if we have injured, and often only after death do survivors realize their neglect. Thus the ghost may be essentially the bad conscience of surviving friends, ever bringing home the consciousness of crime or guilt. There are many fears here because they remind us of our own faults or suggest, if they do not inflict, punishment. Thus the ghosts of those whom savages slay in war must be propitiated, so that they will not come back for revenge. Even the best beloved when dead quickens the conscience, while the ghosts of those we have outraged may take on terrible shapes and do terrible deeds.

A reveniant apparition to a normal, cultivated man who did not believe in ghosts, would be alarming as a symptom of psychic aberration. The victim might very likely prefer the sight of bona fide ghosts to a subjective phantom, and really have the will to believe in its objective reality to save himself from the greater shock of fearing he was going mad. A persistent and reiterated hallucination is sure eventually to

win its way to more or less credibility and certitude, and in such a case belief in objectivity is a form of auto-psychotherapy, somewhat as dreams are sleep-protectors, for it saves us from the confounding sense that our senses are lying witnesses. In such cases a spiritistic faith might prove a benign prescription and mitigate the panic lest reason be tottering. Who shall say that men like Owen, Swedenborg, Luther and many others did not find defence and compensation against illness or insanity by accepting the objective validity of their illusions and prevent the dissemination of their pathological symptoms by accepting the deliverances of their sensory delusions and weaving them into their scheme of things; while on the other hand if they had persistently tried to deny and down them, they would sooner or later have lost confidence in the most basal field of experience and been reduced to confusional mental states.

All these causes, perhaps especially the first four, may combine in various proportions and degrees, while the momentum of social tradition prescribes forms of interpretation as well as feeling-tone to all such experiences.

The fact of death has always called attention more strongly than anything else to the soul or psychology. We see this even in children's fancies that corpses feel cramped in the grave or communicate with each other under ground, that the body will keep growing, that they are lonesome, and this has its highest outcrop in Plato's definition of philosophy as the contemplation of death, in Buddha's life work to find release from his experience of thanatopsis, in the fact that the death fear is sometimes so intense that victims of it are paradoxically compelled to commit suicide to escape their fear. Monks and ascetics by envisagement of skeletons and life in the tombs and all *memento mori* customs which seek to solemnize every moment of life in view of it, and pessimists that imprecate the universe because we have to go through it—all this has contributed to a very intense and very deep belief in the reality of the soul and helps to make us so much more prone to write *hic jacet* rather than *in memoriam*. The impulse to preserve the body or keep it near for a time is also only the reverse side of the impulse to incinerate or dissipate it. The kings of France once remained unburied forty days and by courtly fiction were regarded as if they still lived. Although we are still a little uncertain why the Egyptians embalmed, it was certainly not with the idea that the body should be ready for complete resurrection at the end of their cycle of three thousand years,

although the Mohammedans hold that the very toe-nails and eye-lashes are resurrected, and that the spirit in the next world would forever retain every mutilation. Mortuary customs either tend to dissipate or to keep the dead within the cycle of life. Sometimes a certain part, head, heart, skull or the bone *luz* may be the center of recomposition, but among all cultured people the hope of resurrection does not involve restoration of the body. Mankind will thus never fail to be more or less interested in the postmortem fate of the body itself.

The something that leaves the human body at death has from the earliest times been thought to have some power of independent existence. Heraclitus says the air is as full of spirits as the earth is of corpses. There is much analogy between the expressions of the instinct to put the body away and that to lay or drive off ghosts. Primitive culture holds that they strongly tend to linger near the body. Sometimes widows are plunged under water to drown off their dead husbands before they can marry again. Sometimes tribes turn out *en masse* to frighten away the spirits, as they do to get rid of vermin, rats or clean house. The ghost may be burned in effigy. A window or hole in the roof must be opened at death for the soul to escape, and afterwards closed, the body carried a number of times around the house, so the spirit cannot find its way back. Those unjustly treated or not buried aright return for vengeance. Among many primitive people, some think especially where there is a belief in several souls, tombstones were primitively to hold down the souls of the dead just as the Tiber was turned and Attila buried in its bed, after which it was made to flow back again to keep him in the land of spirits. In Gurney's "Phantasms of the Living" ghosts have their chief power at or near the moment of death. Man was long terrorized, especially in darkness and dreams, by the terror of ghosts, and devised many ritual modes of relegating them to some place appointed, and this was one of the great functions of the medicine man and priest. The living have a domain and their own rights in it, which the dead must respect. The witch makes havoc with this order, bringing back the souls of the departed. Thus many kinds of barriers grow up between the living and the dead. It may be distance only. Again it is a river of partial oblivion or a fire, perhaps a deep chasm, a mountain range, a strip of sea. The ghost world may be above, below, and it is always difficult to get to or from. In general man



does not wish to go to the realm of ghosts, nor to have them trespass upon his preserves.

For each friend we develop a kind of psychic plexus or constellation of associations of which they are the center, and it is these that persist and tend to bring them back, and which burial rites and banishment of the spirits of the dead tend to dissipate. Thus the tears at funerals, scripture, address and even the expense, help to reef in the vivid sense of our friend's personality and to paralyze it sufficiently so that it will not project terrifying ghostly phantoms, to make clustering memories powerless to create morbid images. Hence all such ceremonials are cathartic for by them the folk soul has sought immunity and sanity against morbid hallucinations. Conceptual energy is irradiated by the sense that our friends have crossed a bourne from which they can never return. Thus it was a great step in race culture when a home or rendezvous of departed souls was established, for this was a most successful autotherapy. The New Zealanders erected such preserves for their dead over the precipice of Reinga; Fiji Islanders in their deep and fiery canyons; the Sandwich Islanders in the subterranean abodes of Akea; the Kamchatkans in an underground Elysium; the Indians in their Happy Hunting Grounds; the Greenlanders under the sea; the old Teutons in Walhalla, the temple of the slain, with its columns of spears, roof of shields, seats of armor; the ancients in the realm of Pluto, etc. There are many roads and ushers to conduct souls to their long home. Among some races they traverse the sunbeams, in others the Milky Way, some pass through caverns or over the rainbow bridge Bifrost to Aseir, in Greece Charon, in Egypt Anubis conducted souls across the interval or through the partition. In all this man sought to make himself realize that the souls of the dead were really gone and effectively shepherded in folds of their own. All this shows how tremendously real the belief in souls long was, and how exactly our modern soulless psychology has reversed all this tendency. Instead of being very tenuous, perhaps identified with some part, the shadow, image in the eye, being heart-shaped, etc., it became in many respects more real than anything physical. It is noteworthy how modern thought and psychology itself have tended to dispense with the soul until it seems a residuum hardly worth saving. We are often told that we must drop all conceptions of form, size, color, and perhaps all ideas of its relations to either time or space, that it is vulgar to think of it as phosphor-

escence, electricity, etc. One writer urges that the stuff that now constitutes our soul may be identical with what has constituted other souls, as is the case with the body. Thus psychology has distinctly tended to dissipate the soul and to indicate that it is no more persistent than the body. Nevertheless the old propensity to believe in crassly real souls will not down. Perhaps culture here tends to revert to a low and early stage, and to give it a larger, ampler development or do over again in greater detail and by more modern methods the more instinctive work of bygone stages. It is these old impulsions therefore that agitate neurotics, prattle the small gossip of the other world or burrow in the subliminal regions of the soul. In even trying to convince us of their existence they have relapsed to their old business of seeking to invade again our domain, to evict them from which has cost the world so much. If the ghosts are not baffled in their endeavor to re-encroach on our domain, the life of the world will be handicapped again. Even science in some of its quasi representatives, has had a little difficulty in accepting the inevitable here with joy. In discovering energies, ether and subtle forms of matter, it has opened the door to conceptions of astral bodies, while hyperspace and the world of infinitesimals and the possibilities of the thousand million stars invite the re-crudescence of old fancies. Figuiet conceived etherians, the ether folk, 85 per cent soul and 15 per cent body (instead of being half and half like us), who could endure infinite heat and cold, who are raining into the sun, sustaining its radiation with bodies as imponderable as x-rays and persistent as vortices. These conceptions have the same motivation and are hardly an improvement, e. g., upon Paracelsus' suggestion of the soul of a soul and another of that, and so on indefinitely, with a series of deaths and judgments, a view which has been ingeniously revived, as if the new conceptions of matter were such as to invite an ethereal population since the mind so abhors a vacuum. The final word concerning the reality of postmortem souls was well summed up by Lotze, who deprecated all attempts at proof or disproof, but said it should be left to the unconscious activities of the soul, upon which the immortalities devised for it confer no dignity. What man needs is to focus upon this life and to realize that if there is another the best way to qualify for it is by fulfilling all the best possibilities of this. To this end all the above relegations of ghosts were devised by the race soul.

The momentous step in this evolution after the postmortem world grew real was made by distinguishing between the

bodies of the good and the evil dead, and this distinction Christianity, and even Greece, owes to Egypt. In the "Book of the Dead" forty-two judges reviewed the life of each to decide whether it had been such that his body was worthy of the ditch or of due embalmment, the priests holding the keys of heaven and hell, and using them sometimes to terrify even kings. In other religions a tribunal is erected and judgment given near, at or just beyond the point of junction of the two worlds. Elsewhere judgment is reserved for some great cataclysm, where all are sentenced to a second death or life. We have here traces of the belief in a series of deaths and lives in tentative probational states before final perdition or benizen. Often the evil soul is sentenced to summary death, while the good are given a lease of life eternal. Thus it was that the Egyptians first taught immortality in a definite way, effective for morals. Pythagoras obtained the doctrine here and Plato fastened it upon the Greek mind, and thus helped to prepare the soil for Christianity. Richardson,<sup>7</sup> Beet, Foucart, Maspiro,<sup>8</sup> and Cimont<sup>9</sup> has amplified this influence. The Eleusinian mysteries which opened the Elysian fields in Greece, were of Egyptian origin, as Demeter, while Isis, the worship of which was so like that of Demeter, was gradually Hellenized. Osiris was a man-god, slain and risen and called king of eternity, who had the power to bring back from the dead, who judged and decreed for every soul. Sayce tells us that "the moral teachers of Egypt anticipated the moral teachers of Israel," and Tisdall says that Osiris seemed to the believers in him a real deliverer because he could sympathetically attend the souls on their long and dangerous journey to Elysium.

As to *immortality* in the orthodox sense of the word, if men really believed that there was another life vastly better and more desirable in every way than this, the world would soon be depopulated, for all would emigrate from it, unless fear of the mere act of dying deterred them. At least all the strong and enterprising souls would go. But in fact even those surest of Heaven stay here to the latest possible moment, and use every means at their disposal not to graduate into the *Jenseits*, even though their lives in this world be miserable. Does not this show that belief in post-mortem life is a convention, a dream-wish? Otherwise clergymen themselves would head their flocks into the great beyond. It

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<sup>7</sup> The Old Egyptian Faith.

<sup>8</sup> New Light on Ancient Egypt.

<sup>9</sup> The Religion of Egypt.

is surely not mere duty that keeps us all here. The will to live and evolution impel to the highest, fullest possible life and if this is in another world there we should go. Is not the believer with the strongest faith self-deceived? Is it because all imagery of the next life is either so downright puerile or else barbaric and vulgar that we balk, or it is so vague that it is entirely beyond our power to conceive it, or are the attestations faulty, or the credentials unconvincing? If we were told of a new continent of fabulous wealth and charm, and believed it all, we should go to it by individuals, families, tribes, and leave fatherlands untenanted, although we had to brave dark and tempestuous seas to get there. We should not cling to the old shores until forced to cross, perhaps too weak or decrepit to enjoy or profit by the great change after the landfall. We should not ritually pray against a sudden transit or be called fanatics if we voluntarily crossed the tide because the old world had become intolerably hard for us. We should hasten to go young and in our prime to make the most and best of the new opening. Do not the facts show that the most ardent believers are very far from any really pragmatic faith, or is it rather because the next world seems all enjoyment and we prefer this where achievement predominates, or that we are not weary enough for rest only? Man's nature is active; Heaven is passive. But if this is so, it does not fit his nature, which is the need of doing, and besides, it does not attract even the most idle and lazy or those who seek only pleasure. Even the psychic researcher who holds converse with the denizens of the beyond wishes to go no more than the rest of us, as if in the deeper stratum of his soul he felt that the spirits of his friends were themselves, as Dunlap suggests, in the process of slow rotting or decomposition in various stages. The difficulty that ghost seekers tell us all spirits find in coming back to clear and open intercourse is surely a great handicap on the desirability of this kind of immortality. Why this obvious weakness of faith among the faithful?

The answers are manifold. First, the next life is really a last resort, better than annihilation, but always and to all, less desirable than this. Better fifty years of earth than a cycle of Heaven. It is a kiosk in Kamchatka, which believers have invested something in and fitted out with such comforts as they can, to which they may retreat and find landfall after shipwreck here. Their world of ghosts is really tenuous and pallid, as the asphodel meadows to which Homer's heroes went. But the Greeks said it was better to be the humblest

peasant here than king of Hades. They were more honest with themselves in their belief in a next life than we. Life yonder to them had an inferior degree of reality. Imagination, not faith, was the organ with which it was apprehended. The substance of things hoped for, and the evidence of things not seen were more psychologically adjusted to the power of a wish to confirm the reality of its own creation. It was a product of the Tarn-Kappe, or wishing cap, of the race, which it puts on Sundays, while the wearer strives to reach a hallucinatory degree of certitude and argues that it is intolerable if he cannot have what he so much wishes, and that if immortality is denied his very nature is a lie, and the great Autos which evolved everything is false. But if the consensus *omnium gentium* really failed here, the world would be morally bankrupt no more for denying this aspiration than for denying us many other universal desires, e. g. to live as long as we want to. Is not the phobia of death greater than that of the extinction of the soul?

What, then, are the factors that have so over-determined the hope-wish of personal immortality (which of course may be true, for we are not dealing here at all with the question of fact but with the psychological factors involved in the belief itself)?

I. These are, *first*, the desire to be remembered and esteemed by survivors. We make and keep graves attractive, as we wish our own treated, and the soul abhors the thought of speedy oblivion somewhat as it does extinction. We wish our surviving acquaintances not only to think of us but to think well of us. This desire to live in their kindly and grateful remembrance is only one kind of immortality cult. How satisfying this is to both those who die and those who live is seen in the fact that in Confucian lands ancestor worship vicariates for faith in personal immortality. The saint might not exchange his hope of Heaven for the certainty that in due time he would be canonized, or perhaps the scientist might not for the assurance that he will be perpetuated in marble or bronze or stand in some temple of fame. Yet would not this mundane in some degree compensate for the loss of celestial perpetuation? Would a fervent believer, however devout and full of piety, not feel at least some degree of compensation for not finding his name in the book of life if he knew it was to be inscribed among the forty immortals of some academy? Studies of the dying show that in their last conscious moments they are prone to turn to and dwell on their friends here far more than

they do on anything or person in the next world, believers though they be. The enshrinement they chiefly want is in the thoughts and feelings of those they leave behind. Conversely, too, those who die alone, friendless, among strangers, or with the execration of survivors, cling the more to the rehabilitation that death itself, to say nothing of another life, will bring them. Thus these two have no small degree of compensating or inverse relationship. We may go still farther on the basis of questionnaire data as I interpret them, and say that the believer who is assured of Heaven would anticipate less enjoyment in it if he knew that all about him thought him doomed to Hell, or even if they all believed in extinction. We want survivors to think we are enjoying the very best the universe can provide for its favorites because in that case living friends will think more highly of us, that we have obtained the diploma of the cosmos. We are in and worthy of a place in the world's highest social aristocracy beyond the Styx. Our blood is heavenly ichor. We have stood the great test and graduated *summa cum laude* from the terrestrial curriculum. If we were early Christians we should begin to put on airs and affect the manners of Heaven here and gloat over and inflame our ego by contemplating the non-elect or lost, as if to get all the greatest possible satisfaction out of this relation by anticipation, for of that at least we can be sure. Thus the sanctified soul striving to impress its own evaluation of itself upon even lost sinners, unlike the ancient sage who would prefer that people denied that he ever existed than to think him bad, prefers to be hated rather than to be forgotten. Thus, in fine, if all knew that they and all their good deeds would never fade from the memory of a grateful group of survivors and their descendants, they would feel the conviction of a continued conscious personal existence beyond the grave a little paler, for they would lose one of its preforming determinants and reinforcements. Therefore those concerned to keep alive the faith and hope of another life yonder should foster any agency that assures them that all who die be not forgotten, but be kept tender and green in memory. Here, again, is a pedagogic theme but little worked, e. g., the value of funeral services, eulogies, commemorations, and all forms of memory perpetuation. There should be those who sum up effectively the good lessons and meanings of every life when it closes, as a kind of mundane judgment day, that no good influence be lost and no warning fail of having its due effect,—a court of the dead to pass impartially upon each life as it sets out to sea. The

moral educative effect of the installation of such a new culture institution would be incalculable. As we censor books, test for eugenic marriage, etc., so if all knew that at their death a supreme and impartial tribunal would pass upon their lives in the interests of the common weal, at say a certain limit of time, even if the verdicts were secretly filed save for a few or even for another generation, or given only to those who had most right and need to know, then ethical culture would make a great advance. The fear of death, instead of wasting itself in abortive ways, would be set to work for the normalization of lives. The judgment of our friends when we pass away would turn to great efficiency the instincts of love, gratitude and appreciation.

II. The *second* mundane surrogate for transcendental immortality is doing things that will affect those who survive us here or the perpetuation of our own will and works whether or not we be personally remembered. We write last wills and testaments, erect buildings, establish organizations, start movements, write books, create works of art, invent, transform the face of nature. In all these fields of human effort, one partial motive, which may be more conscious or more instinctive in all degrees, is the impulse to be a power in determining the lives of those who come after, even though we are unknown. All in all, this is less selfish than the former wish to be remembered, and though an egoistic element is often present, it tends to merge into a disinterested desire to make a part of the world better in some way for our having lived in it, whether we are known or not. Sometimes, indeed, anonymity is striven for and the individuality of the benefactor is hidden. The phobia here is that we may have lived for naught. Here the idea of God as an all-discerner who sees merit and vice and rewards or punishes in secret co-operates. Yet such secret service to the race, with no conception of any knowledge or compensation here or hereafter, has a unique charm of its own. Scientific discoveries and beneficent inventions have been given freely to all without any personal benefit in return or even without a personal label. True love sometimes lavishes every opportunity and joy upon its object with no stipulation of gratitude or even recognition in return. The possession of wealth compels attention to this field of the *immortality of influence*. Unlike the beggar, the multimillionaire must forecast if he would try to shape the future, and an endower and philanthropist, even if he attaches his name to a benefaction, is predominantly

concerned for the good of mankind through the ages to come and knows well that he will be forgotten or will abide only as a name. Jubal invented music and wandered afar, until in his old age he came back and found a great festival in honor of his art and his name, but could not identify himself and was cast out as an imposter. "Jubal's fame and art filled all the sky as Jubal lonely laid him down to die," happy because he had done the race great service. To love and serve man is far higher than to love and serve God. We can do nothing for Him save in this way, and He needs and accepts no help from us save thus. Men come and go, but influence and institutions go on forever, and those who start them share all their mundane deathlessness long after they are forgotten. The cup of cold water illustrates the ideal of the gentleman and lady born and bred, best attested by the desire that others be happy and not that they themselves shine, be aggrandized or have pleasure. This is the most ideal conduct and appeals most strongly of all things to the two great and ultimate standards of conduct, viz., honor and the approving conscience, and as we achieve this we belong to the order of the immortals and have triumphed over death. Desjardins, the founder of the order of the new life, says in substance that never are we so impelled to snap our fingers in the face of death and despite all his pomp and terror defy him to do his worst to our body or soul as when we have just performed some such act of pure but passionate devotion to duty. Then only we truly feel that no evil can befall a good man living or dead for this is a cosmos that is moral to the core.

III. The *third* killer of the death fear is children and posterity. To die childless, knowing that in us heredity, that began with the amoeba and has come down in an unbroken line, dies, sharpens the sting of death, while on the other hand, to have many well born and well reared children to rise up and call us blessed is one of the best antidotes to its baleful psychic virus. In general every animal, man included, lives as long after the age of maximal power to propagate, as its offspring require to become mature, so that the prolongation of infancy means the prolongation of old age. The first and foremost duty to mankind is to transmit the sacred torch of life undimmed, and to give the maximal impulse and right direction to the nature and nurture of offspring, and effectiveness in this is the best test of every human institution. The true parent lives not only for but in his children, is the ancestor of their souls as well as of their body, and even his



belief in a future life is good or bad according as it affects this. We may feel this life incomplete, unfinished, in need of a supplement, because possibilities are unrealized, but we shall not feel this so much if we have children, while the dread of the inevitable hour is as a kind of second or dual death for the childless because not only they but their line dies out in them. Yet on the other hand, they have less ties and so less to lose, especially if they have not loved and been loved, even if they feel that they have in a sense lived in vain. What parent was ever so world-weary, so ardent a believer in postmortem joys, that he would not rather live on here and see his children's children thrive than to go hence? Such have no time to develop morbid fears of Lethe's waters, and in the great peace beyond they perhaps expect to find their chief joy in contemplating the fortunes of the fruit of their loins on earth. We have seen how real death thoughts begin with the life of sex, and when the latter, if it has been normal and happy, comes to an end, death has already begun, and we are advancing deeper into the shades of the dark valley, so that there is already less to lose. Normal senescence is more poised, cool, contemplative, and as the end draws nearer, life means less, and so the thought of it loses force and expectation makes less demands on immortality, so that death is less tragic and less inconsolable and even such wish-born, or semi-poetic views as reincarnation or eternal recurrence, loosen their hold.

IV. As to the good old doctrine of personal immortality, we cannot escape the great law that the next life is inversely as this. When men are wretched here the future becomes a refuge, and it grows not only actual but attractive, and conversely when this life is rich and abounding, the next world fails. No Christian age really was ever so heedless of the latter as our own. It has in most intelligent, prosperous women, and especially men, lapsed to little more than a mere convention, a trope or fetish of an effete orthodoxy, and hell is for most of us only a nightmare of the past, a childish fear phantom. Our actual *modus vivendi* is as if another life did not exist and death were the end. No priesthood can longer make man content with misery in the hope of compensation hereafter. All now make the most of this life as all they are sure of on the principle of one life at a time and this one now, save only in crises of great emotional stress that stir up the ancient foundations of our psychic life. Or when they modulate over into some secondary personality

that a religious thought or Sunday brings, that turns their thoughts to the future for the nonce, they have for the most part only the flicker of a hope that death is not the end. This at least is pragmatic. Yet extinction is very black by contrast, in proportion as life here is bright, happy, absorbing. Thus the death-dread is in some respects growing intense and yet it is being banished from consciousness. The only attractiveness about it is that it is rest and peace. Our tropes of it are more and more borrowed from sleep. We dread the act of dying and refuse to indulge hope or fear beyond, and now that this may be made and often is painless, we conceive it as only the negation of life, so that if it never was more dreadful than now there was never more diversion from its closer envisagement or more resignation to it. Though most suppressed, it never was so potent a factor in governing conduct of life. We nurse ourselves into maximum of health by every known method, wish to intensify and prolong our lives, make our wills constantly if we have property. We take a chance at saving our souls in church, although it remains bad form to discuss such matters seriously. The million childless homes in this country bring death nearer, as does the hypertrophy of the sex function, and the dread of the diseases that attend it, that have poisoned the arrows of love. But the real treasure of the soul is more and more laid up in this world and the growing phobia of death, which is the ultimate and most generic of all phobias, has had so many and effective psychotherapies in the past that there are doubtless more to come, although some of the old ones seem passing. If this tremendous power is not checked, who can conceive the many uses to which it can yet be put, serving useful ends in the world's work?

All depends on how rich, full of present joy and future hope, life is. Even a little physical malaise, disappointment, discouragement, ennui, the death of a friend, makes us, as we saw above, a little less alert in our precautions against accidents, a little more likely to catch an infectious disease or a cold, or to succumb to an illness, a little more reckless in our diet, sleep, personal hygiene and regimen generally, more prone to take risks because, unconscious though it be, life seems a little less worth living. Many supposed suicides are not so at all but are only accidents due to a temporary reduction of normal care and caution, which may be caused by the most transient, slight and partial depression. True, great sudden joy may make us temporarily careless and so may unusual absorption, but many have died prematurely

from mere inadvertence simply because they momentarily lost heart and their vital spirits sank a few points on the scale of euphoria, though still remaining far above zero. Every pain checks, if ever so little, the momentum of the will to live, and every reduction of it below its maximum or superlative degree slightly augments the chance of death. On the other hand, when this is at its optimum or flood tide, man is wonderfully immune and recuperative from every kind of injury, body and soul, and the higher up on the hedonic scale he lives, the more impossible it is to fear or even think of death. Every new, legitimate, higher pleasure, therefore, is a life preserver and prolonger. When Calicot, perhaps the best type of a clown, was asked how he wished to die he said, "Breaking a blood vessel in a fit of inextinguishable laughter would be the ideal way." "If," he added, "I had to be executed and could choose my own way, I would prefer to be tickled to death." Others have said in the consummating act of love. Few develop any very definite ideas of how they would prefer to die and of those who do so, few would choose such a mode of exit, but on the above view it is plain that to dwell much upon it is a bad sign, for it shows the sinking of our mercurial vital spirits below the limit of normality. Since our business here is to live and not to die, mental hygiene consists in keeping high up on the vital scale, staying always as near as possible to the top of our condition, being able to answer the most universal of all questions, how we feel, with an unctuous "Very well indeed, I thank you," and not with a tedious portrayal of dysphoric symptoms. If we keep up the grand old fashion of prayer, it should not be on the agnostic pattern, "Oh, God, if there is a God, save my soul, if I have a soul, and if there is any salvation," nor like the beggar's plea, "Oh, Lord, give me something good, and all that is best, although I deserve nothing but the worst," as though we had a pull with the Divine; but rather, "Keep me always as far from death and the fear of it, which is the *summum malum*, as possible," for that is on this interpretation the *summum bonum*.

As to the *relations of these four immortalities*, nominal, influential, plasmal and orthodox to each other, new and profound as the problems are, geneticism already has a few insights into the dynamics of the great racial soul which lives, moves, and has its being in us, but which is more concealed and masked behind consciousness than revealed in it. The fact is that either may in some sense vicariate for any or all of the others. (A) He who is chiefly intent on perpetuating

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his name, is gratifying the deeper instinct far transcending the limits of his own personal life. To know that his name will be remembered may have atoning and consoling power enough, even to make up for the absence of posterity or for the conviction that death means extinction; while conversely the prospect of death in utter obscurity and of being completely forgotten tends to reinforce one or all of the other three immortalities. Were we to rehabilitate hell in a modern sense, one of its horrors would be a sentence of summary oblivion even to our friends: "Let his name be forever tabu from mention or even memory." Yet most of us will, some sooner, some later, fall under this sentence, despite pathetic efforts to cut our names deep upon the most durable gravestone or to affix them to great institutions as founders or donors, or even on the title pages of our books.

(B) As to anonymous influence, that we are all sure of in a sense, for every life makes some durable contribution. The physical and social worlds are certain to be eternally different, even though infinitesimally so, for our having lived. Our very physical life leaves its swerves in the distribution and direction of matter and motion, and in even this poets and sentimentalists like, e. g., Richard Jeffreys, have found gratifying vents for the instinct of perpetuity in the thought that his body would at least be a compost and would not be buried so deep that the roots of vegetation that grows on the grave could not reach it. In the social field we have endless illustrations where service involves more or less self-immolation. A case in point is a woman I know who, having long led a most disinterested and self-sacrificing life, was told that God, who knew all her merits, would reward her in the next world. She replied that she never had any convictions or interests in another life and had been too busy doing good to think about it. If another life was in the order of things it would be all right, but if annihilation was the law, that too would be just as welcome, for she had found her pay in the deepening satisfaction each day's work brought to her. She had no children and wanted no outer recognition but was supremely content to know that her good deeds registered in others' lives would follow her, and nothing else really counted in her scheme of life. The point is that in other ages or environments her instinct for doing good might have found its chief expression in either of the other forms. Again, criminology shows us that some perverted souls commit in secret the most colossal crimes from the same *Geltungs* pro-

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pensity. Because they cannot be potent for good they make themselves so for evil. Anonymity is not unknown, even in science, although far rarer than in philosophy and religion. Many an epitaph is written only on the hearts of those who never know their benefactors. Here too belongs the strange passion of pseudonymity in authorship in the earlier Christian centuries, in which the writer sacrificed personality to his cause, which he felt to be infinitely greater than himself. What we call environment in the largest sense, and much of civilization, great institutions like Catholicism, etc., are made up in no small degree of influences originated by those whose names no history or *acta sanctorum* has preserved, but which would never have started without this deep *este perpetuum Anlage* in the soul.

(C) As to plasmal immortality, who knows how much of both the above impulsions was not ultimately motivated, could we only know their deeper genetic roots, by the original momentum of the instinct to make the world better for posterity? If so, they also are now broken erratic trends, forgetful of their source, which is the nest-building instinct, so irradiated and sublimated as to have lost orientation both of their origin and their goal. The first constructions in the animal world were nidifications. The first property consisted of food accumulations for self and for young. The first animal societies were either for mutative or stirpicultural ends. The first and chief examples of self-sacrifice were made for the young. Everything in this world is good that squares with the function of parenthood, broadly and genetically conceived, and all is bad that contravenes it. Psychotherapy is slowly leading us to the astonishing new insight that aberrations of the life-transmitting and young-rearing propensities are the chief root of about all mental and nervous abnormalities and that the rectification of this function has marvelous therapeutic efficacy. The race is immortal, at least backwards to the first protozoan and indefinitely beyond, and so in the future our race is at least immortable to the cosmic end. If we are the tips of the twigs of a vast buried tree, these twigs may become themselves roots of a yet greater one, and a true superman may be born in the line of any of us. Thus perhaps all the other immortalities have their chief dynamogeny in the instincts of parenthood.

(D) As to the venerable doctrine of personal immortality, of course it was selfishness transcendentalized to subordinate every other goal of life to ensuring our own happiness in a

post-mortem and perdurable world, and one has only contempt for the squalid ascetic who made his life poor, mean, ignorant, unsocial, with the prime aim of saving his own soul. This doctrine, crude as have been most of its forms, is now coming to stand forth, however, in a new light. It was an ugly cyst or cast that enclosed and sheltered through hard and dark ages a precious and beauteous thing. As now revealed by analysis, it simply expressed in a crude form man's ineluctable conviction that his own earthly life was insignificant compared with its larger meanings and possibilities. It taught subordination of the individual to the larger whole, toward which it gave him a correct *Einstellung*. The close attachment of this doctrine to the ego was incorrect, but necessary from the standpoint of race-pedagogy. The transcendentality it ascribed to the larger self was also inevitable because that was the only way in which the greater life of the race soul could be described or comprehended. Even if it is a fact, it is far more important as a symbol of the larger, higher life that is accessible to all who seek it earnestly enough. It was eloquent of the higher powers of man, even though its temporal perspective was awry. It stored up and conserved the psychic promise and potency which is now again flowing over by transfer to the other vents of the immortality-instinct. It did not say all it meant or as it meant it, but nevertheless it was a pragmatic masterpiece like all the greater creations of the folk-soul. It went home straight from the soul of the race to that of the individual, and if it over-stressed individuation for a time, that too was needful in its day. Had man not so long or so intensively believed in the great work of saving souls for the next world, he would now be less effective in saving them in and from the evils of this world. Had he not so cherished the conviction of a future heaven he would have lost much of the momentum and psychic energy of soul that now strives to transform this world into a paradise by bearing and rearing the most and best children to inhabit it. Thus far, at least, we are getting new glimpses into the laws of the transposition of psychic trends into their kinetic equivalents under many different forms, but with a persistence of content.

The second law at which we thus arrive is that if either one of at least the first three immortalities is excessive or defective each of the others suffers more or less arrest or perversion and that the hypertrophy of either dwarfs and stunts the others. We must also add that insufficiency of the fourth form of the conviction also results in loss or error in

the others. The doctrine of personal immortality has still a very important rôle to play for children and adolescents. Pedagogy is now sufficiently advanced (greatly helped out, to be sure, by pragmatism, which is nothing less than the pedagogization of truth), so that we may boldly affirm that the highest truest truth is that which works best rather than that which satisfies the criteria of abstract logical reason. Hence we may plead that this doctrine is an educational necessity and should be inculcated to the young irrespective of the creed or scientific belief of their elders. It is also a therapeutic truth and fact, and to doubt the above principle now is only another illustration of the sad fact that cultivated adults in this country have unprecedentedly lost touch with youth. This is connected with the deterioration of the higher instincts of parenthood. For geneticism personal immortality will remain, as it was for Kant, a postulate of practical reason. It is not only a power-house for energizing the other immortalities but a very present help in regulating the lives of the young at just that stage when feeling and impulse are at their freshest and strongest, although least understood and most in need of idealization. Thus genetically nothing is more certain and with the abstract validity of the concept the psychologist is not concerned but leaves whatever view the reader may hold untouched.

V. There is a *fifth* immortality, more apart, and uncorrelated with the others, that is motivated by the call and the lure of the infinitesimal elements which science now finds at the basis of the universe. The atoms of Dalton are now known to be very complex, each, in Oliver Lodge's phrase, being a planetary system of unimaginable minute corpuscles, one one-thousandth the mass of an atom of hydrogen, and if they are solely electrical "their size must be one millionth of the linear dimensions of an atom." Their size, he adds, is to an atom like that of a dot at the end of a period of print to a large theatre. Their different groupings and motions constitute the chemical elements, so that matter is dynamic and the universe is at bottom not some unknown hypothetical protyle, but the familiar electrical charge. Thus he says inorganic matter may contain some germs of the thought that is in man and mind. Matter is fecund, always producing organic forms and thus electrons, the basis of all, as is revealed by the study of radium, in the cathode rays, have given us a most attractive new conception of the roots of the universe. Mezes too conceives one mother substance

essentially active and homogeneous of mind, thus freeing us from the hopelessness of dualism and giving a monistic view. "Nowhere is utter death to be found," but there is an unbroken gradation "from the corporative union of electrons in an atom up to the aggregation of men in society and possibly further still." On this view death is not only non-existent but inconceivable. Larger, more complex and temporary aggregations are reduced to the simpler and more perdurable ones from which man has developed. Matter is not dead but far more intensely active than mind, so that this is a dynamic or spiritual universe, and the student of the ultimate constitution of matter is, little as he suspects, studying immortality or the basis from which all the orders of animate nature arose and into which they will all be resolved. So lawful and continuous are all these processes that the new physics and chemistry are really investigating death and the future state. True, our loftily and totteringly unstable consciousness and even our brains have a less direct sense of the processes that underly all their activities than even of the activities of the digestive processes or of the phagocytes. While we deem evolution upward there is another sense in which it is a fall or a series of departures from more durable simple and elemental processes, so that the gain is not all one way, and katabolism has its own attractions. If all the effects of my life ultimately leave the electrons that compose me different in themselves or in their relations to each other from what they would otherwise have been, this too is a certain form of survival which may come to afford not only consolation but positive satisfaction. Even if my life is only a temporary swerve or a transient eddy in the great current of dynamic energy, my ego shares all its eternal persistence, so that although inconceivably changed, I am without end, as I am without beginning. All stages and processes of death are but the loosening of associations from the most complex downward. The disintegration of our elements into those of the larger whole is a harvest home to the cosmos from which we arose, with perhaps increase and not decrease of the sum total of good, although disposed of automatically instead of egoistically determined. This unselfing or fusing into all we flow from is the direction which love, whether of man, woman, the race, animals or nature itself, as well as every subordination of self to others and the world, inclines us. In those in whom the last vestige of self-love is eliminated the *nisus* of self-sacrifice has fully attained its goal. The world is still far from seeking this ideal but every abate-



ment of the old zest for a personal future state is because the consciousness of civilized man is being slowly swept on by deep phylogenetic tides of which our poor psychology as yet knows little. Should such a conception of the end of man ever become general, it would probably be surprising how many of our present religious phrases, litanies, eschatological tropes and symbols, would remain and be filled to overflowing with new fresh meanings. The religious instinct has always been vastly wiser than it knew. Such a view of course will spread very slowly and can hardly be expected to pervade the masses. Hence we must continue to cherish as sacred even for ourselves as long as they have any vestige of moral, emotional or other value, and still more for the young and the less advanced in insight, every ancient mode of considering man's immortal longings and every argument, simile and analogy, not only because these make for righteousness as they are and as man is, but because they are all true so far as they go, that is, they are in the right direction. Their defect is only that of half truths, incapable of containing all the pregnant burden of the new dispensation. Thus if at death the psyche is disintegrated as much as the body is, and the dissolution goes down to molecules, atoms or basal forms of energetics, it is not absolute. The difference is like that between the mountains and the sea level when compared with that from the surface of the earth to its center. In this world of elements nothing is lost. What we have called matter is, as Hering and Semon have told us, memory, and despite the dissociations of death our individual experience and personality may leave immortal marks in some *materia prima*. The world beyond is like an ocean to an ant accustomed only to its ant-hill but floating out to sea on a straw. We do not dread sleep; why not welcome the great rest? When we pass to the subconscious it is vaster than the conscious as biology is greater than psychology, folk lore than philosophy. We want to feel the elemental forces and powers that are not ours, to be inundated with a strength not our own, to fall back to everlasting arms. Back of Christianity is another meta-Christian, meta-human religion in the love of nature, and old men ought to grow more interested first in animals, then in plants, then in the inanimate world. This view of course harks back to pantheism, to Spinoza's conception of death as merging into the absolute substance and relates Ostwald and Parmenides. It is a modern form of the Indic absorption cult, as revised by science. We are now coming to the belief, if we follow the Durkheim school, that

all primitive people had a premonition of it and that back of fetichism we shall always find mana cults. This general vein or trend of soul also reinforces Wordsworthian conceptions of childhood and is in sympathetic rapport with all emanation theories as well as with Om cults, which for upright upward gazing men, addressing the sky as "Our father in heaven" is only a vastated navel gazing orientation toward the source of things. Schleiermacher's conception of religion as a feeling of absolute dependence, which he considered in his "*Reden*" as pure pantheism, he sought to use as a source of personal consolation to the young widow of his dear friend, the young preacher Ehrenfried von Willich. When in her first hours of bereavement she poured out the whole passion and tumult of her soul to Schleiermacher, full of pathos and of the naïve sense that her dear one still lived, loved, and awaited her in the unseen, saying that in the grasp of her sorrow she could not possibly feel that all the past would go for nothing and that his soul would be resolved back and melted into the great all and never come to recognition again, he replied that this sense of melting away should bring her no grief for it was merging into the highest life in the divine whole to which we belong; it was merely putting away the pretensions that we set up for ourselves as if we could be our own. "If he is now living in God and you love him eternally in God as you loved and knew God in him, can you think of anything sublimer or more glorious? Is not this the highest end of love, in comparison with which everything that clings only to the personal life and arises thence is nothing?" Mailänder, who held that a divine being died in giving birth to this world, and that all its processes point ultimately to Nirvana or are forces of self-destruction, since everything is traveling the road to death, held that the desire for it was the universal motive and that we are unconsciously seeking death in all we do or say. Man must dominate the world by knowledge in order to be able to enjoy the prospect of annihilation and to obtain the full will to die. We must indeed be resolving ourselves back into primal energy, which is nothing only in the sense that it cannot be defined, the absolute of his "negative eudaimonism." Renan said, "We shall live again only by the traces we leave on the bosom of the infinite" and that, hard as it now is, highly cultivated minds will rise to find consolation in the thought of merging in the all. Meyer-Denfey says that no part of the soul can be lost any more than can any element of the body, and that our actions leave traces on the absolute "so much the deeper

as our life has been fuller." The new Nirvana or immortality of Metchnikoff is "the reunion of the atoms of the individual with the life of the whole of humanity and through that of the cosmos" and only by directing our thoughts habitually along this line can we overcome the fear of death.

A variant of this view, represented by James, Myers, Schiller and other psychic researchers, is the view that premises a higher pre-existent absolute mind behind the conscious and phenomenal world. It may be a sense of a world soul that permeates and percolates ours. Schiller in his "Riddles of the Sphinx" thinks that matter limits and regulates its activity. Brute brains transmit but little and are in lethargy, while men with higher organisms transmit more as in somnambulism. This view is like Schelling's that so-called dead matter is the sleeping plant world, that animals and men are still more highly potentialized, and that conversely as we go down the scale of existence mind becomes more and more extinct. Nature is visible mind, and mind is invisible nature. The evolutionary series are stages that the absolute mind produces in developing its own self-consciousness and imaging itself, which is the goal of creation. For Schelling mind and nature were at root identical, but Schiller is more dualistic and regards the body as "a mechanism for inhibiting consciousness, for preventing the full power of the ego from being actualized." He says we must explain the lower by the higher and not conversely, that during life we must drink the cup of Lethe, so that "with our brains we are able to forget." Myers held that the normal human mind is in rapport or direct contact or continuous with a larger consciousness of unknown scope. This connection, however, was below the threshold of our consciousness, which is high. So far as this larger soul finds expression in us, we call it our subliminal self. Thus materialism is refuted, for the brain does not produce or secrete thought but obstructs it like a bad conductor, so that when the thought currents of the great Autos pass, the nerve glow, phosphorescence or incandescence caused by the resistance of the brain is what appears to our fragmentary subliminal mind as consciousness. So too James thinks thought a transmissive function of the brain. "Our brains are thin, half transparent places in the veil" through which the great life of soul "breaks into this world in all sorts of restricted forms." In some brain states the barrier is greater and in others less resistant, as when a flood of spiritual energy pours forth. The brain state may vary independently and the supernal current with it. The stream of

consciousness may even be shut off for the latter is not generated *de novo* in many places but pre-exists behind the scenes. Thus we have a changing threshold and a variable permeability and willingness to reveal the influx from the great ocean of mind. At death these currents revert to their sources and personal identity seems to require the persistence or at least some of the same restrictive conditions and limitations. But here James stops with characteristic abruptness before this inference, which is the crux of his entire contention. Instead of a world soul in the background which our brains separate and organize into finite forms, he says there may be only mind stuff pre-existing in minute and disseminated fragments, which our brains mass, concentrate and combine into human souls. On this view the fragmentary soul elements, whether they be combined in a human or even animal ego or not, must also be immortal for the same reasons that we are. We need to be told what are the effects of these combinations, for this view, which ordinary pantheism provides, does not tell us. Again they might be incorporated into higher beings which would be resolved back again into us, on their way to more elemental forms. There is thus a veritable immortality for the elements of which our psychic life is composed and at death we leave behind plastidule, atom or yet more elemental souls in indefinite perspective. If our soul is the mouthpiece of an absolute soul as the word *persona* is often interpreted to imply, inadequate though it be, it is still to those lower more morselized souls somewhat as the divine or absolute mind is to us, and as their voices are absorbed in us, so we are in infinite being. We are bundles, a vinculum or parenthesis of more ultimate elements that precede and will survive us, but we are helping these immortal components on to their own goal, so that the real value of our life is theirs, not ours. This view, however, assumes not a physical but a spiritual and more or less conscious background to the universe. If the subliminal functions are most immortal, dissolution might be desiderated, for organization obscures the ultimate reals and the massing of lower monads involves a larger sum of arrest, so that perhaps our lives really hinder rather than help the work of redemption. As in chemistry the more complex combinations are unstable and tend to disintegrate, so the psychic compounds we cause might persist a while, but the lower and simpler ones will outlast them. The function of our earthly life would then be akin to that of careful breeders, who may leave their permanent mark upon the vegetable and animal species and varieties they have origin-

ated long after they are forgotten. But this conception regards the background of the universe as conscious and death is not lapsing down but up the evolutionary scale. It is idealistic and conceives consciousness as both the muse and the goal, and so in a sense is the opposite of the above scientific view, but like it has close correlations with the older pantheism.

VI. A sixth group of proofs or forms of conceiving and cultivating immortality is inseparable from idealism and goes back to Plato. He found men confused and sought by cross-examination and induction in the psychic field to attain a few fixed ideas that the soul could anchor to in the sophistic flux, minds be thus drawn together and Greece saved from disintegration as the old belief crumbled. His Socratic midwifery brought to birth certain forms and basal concepts, *Begriffe*, which were thought to be the eternal patterns of all things, by participation in which everything became real. These Aristotle and many later writers elaborated and defined as a table of categories or innate ideas. They were also interpreted as *summa genera* or fixed species or types in nature. The strength of these two positions, which Locke and Darwin respectively attacked, goes back to Plato. The species and entities of the scholastics, which underlay even the doctrine of the eucharist and not only nativism and apriorism but all forms of philosophic realism as well as absolutism, metaphysics, ontology, rational transcendentalism, the passion for deducing from presuppositions data elsewhere derived, and the Stoic and Kantian conscience, all rest upon the assumption of definite and abiding norms in nature or mind, which are simple and indecomposable by psychic analysis, and with which all sound thinking starts and stops. Thus the doctrine of ideas has been the key not only to philosophic orthodoxy, but to much of the thought and most of the great controversies of the world. Theologians, Descartes, Spinoza, Fichte, Hegel, mystics, illuminats, rationalists, scientists in their quest for constants and laws of nature, and the codifiers of Roman law—all were inspired by this belief in ultimate and attainable ideas.

Now all noetic theories of immortality agree in holding that it is attained when the intellect intuitively grasps one or more of these ultimate truths and thus partakes or participates in their perdurability. They are so high and abstract that Plato conceives philosophy not merely as the withdrawal from sense and the world toward the solitariness of the absolute, but the active practice of death. They are, as it were, somehow the

inner constitution of the mind of God, to know whom is eternal life. The great bliss and peace of what Aristotle describes and praises as the theoretic life is interpreted as a foretaste of heaven, as indeed the Phaedrus intimated. Thus the love and struggle for true knowledge is the desire for incorporeal existence.

Wordsworth's ode on the intimations of immortality describing in us "truths that wake to perish never," "high instincts before which our mortal nature trembles like a guilty thing surprised," is based upon the doctrine of reminiscence. The soul attains or reaches great and eternal verities; thinks God's thoughts after him; attains the absolute and unconditioned; experiences love, which is felt to be stronger than death; finds a sense of pure autonomous oughtness welling up within itself; comes to consciousness of, e. g., the idea of God or of the greatest, best being; envisages a beauty that is transcending and seems to take the mind above time and space into its pure being; has a vision of the eternal world of categories; attains the *summum genus* of abstraction and generalization under which soul and body, life and death, are alike included. The implication always is that in these transcendental, noetic experiences, the soul outsoars mortality. This occurs either by the subject parasiting onto its object and becoming absorbed in it, thus attaining its perdurability by being lost or swallowed up in the ecstasy of contemplation as if the subject acquired or even became substance in this act in a unique way, or else in these ideas finding conscious bearers as reflectors, containers or co-ordinators for themselves. Sometimes the argument implies that because the soul harbors these great thoughts it thereby itself acquires a new quality of permanence. All these views agree in holding that immortality may at the very least be begun in the present life, and that higher speculation upon the categories, which Hegel declared to be the intimate constitution of God as he was before creation, is an experience of a distinctly supernal or celestial type. Such knowledge is not attained by reason, but is usually revealed in aperçus which cannot be proven by argument, but which carry in themselves their own immediate evidence. The rapt seer cannot demonstrate and often cannot intelligibly impart his mystic insights.

Important as is the rôle of this large class of putative evidences, they must from their very nature always be inconclusive. Knowledge is not a participation in this sense. A being of low order may know one of a high order and be

strongly attracted, but the chasm between subject and object remains unbridged. We know, love, and are perhaps ravished by wealth, beauty and power, but do not thus attain them. The further epistemological assumption that the world of ideas is itself a projection, as idealists assert the world of sense to be, is necessary, and even that colossal postulate would not suffice. If these ideas are ejects only, their perdurability is nevertheless forever undemonstrable. On this hypothesis, the mind creates its own saving principles and is in turn saved by these its products. Such a soteriological method is not only indirect, but begs the whole question, which becomes again one of fact. Does or does not such a process occur in our psychic nature? The only possible support for such an hypothesis is the degree of coherence of its own parts with each other and with experience.

Closely connected with this view is that which assumes that because we have the idea or the wish of immortality and because this is so generally implanted in human nature, it is true, or else the latter is a lie. Of this class of proofs the most common are those that urge it because of its practical utility for morality. In the other worldness of the early Christian centuries, when eschatology was more developed than cosmology, fear of hell and hope of heaven did doubtless perform a very important function for virtue, the progress of which was no doubt advanced by these artificial and external supports. But they were given undue weight and were relied on long after their function should have been progressively replaced by the conception of virtue as its own reward. Chateaubriand and even Luther thought that the chief motive of morality would be gone if there be no future life. Andrews Norton held that there could be no religion without it. Even Theodore Parker, emancipated as he was, said in substance, "If I perish in death, I know no law but passion." Chalmers urged that without it, "God is stripped of wisdom, authority and honor." Walt Whitman said, "If rats and maggots end us, then alarum, for we are betrayed." Human nature has been called a lie and God a liar if there is no future life, and those who do not supremely desire it have been called in reality already dead. The supreme effort and tension of many persons and communities has been to escape eternal pain and win eternal bliss, or to save their own egos hereafter. If, says one, our souls do not "hold the latch-string of a new world's wicket, then good-by, put out the lights, ring down the curtain. We have had our turn, and

it is all so nauseating that even suicide is a welcome escape." I have looked over but very few of Alger's<sup>10</sup> five thousand titles, a very incomplete bibliography of the subject even to that date, but have noted quite a list of desperate things that would happen in the world and that individual writers would do and of imprecations on God, his character and the nature of the universe, if this hope were proven false or if the complex net of theories we have flung to the other shore should not hold. All virtues—piety, honor, integrity and civilization itself would perish; men become brutes; God a malign fiend gloating over the unbridled lust and supreme selfishness that would slowly sweep man from the earth. As a boy I once heard a clergyman paint a picture, still vivid in my mind, of the last human survivor of the war of all against all that would break out if it were proven that there were no future life, trying to gratify his three base appetites for slaughter, gormandizing and sex in the last hours of his spent and flickering life. Fear and hope have been powerful motives powerfully appealed to in the constitutions of all the heavens and hells that men have depicted, and have been made very real and present even in our own times. Tupper thought the moon was visible hell waiting to receive the lost; and Isaac Taylor deemed the sun heaven, though a later contemporary theologian has called it hell, adding that its dark spots were the shoals of the damned. Every great comet of the last ten centuries, it is said, has been called hell making its rounds to gather in its victims. These views are even more vivid and modern than those of the old Saxon catechist who pronounced the sun red because it looked on hell, which from Dante to Thompson's dreadful night has been thought to make for virtue just in proportion as it was hot or cold or otherwise terrible. Such views all assume that man is by nature corrupt and kept decent only by reward and suffering. If future pain is not assured, a flood of evil, now held in restraint, would deluge the earth, and if there is no heaven chaos breaks loose as if a great firm were found bankrupt when pay day comes after a long, hard term of reluctant service.

All this rests upon the assumption that a future life is proven because it has developed virtue, and because it is such a persistent belief that, if false, nothing in the soul is to be trusted. Neither of these assumptions has any logical value as proofs. Even the good Bishop Butler argues that men must be prepared to find themselves misled. "Light deceives, why not life?" From childhood to the grave and from

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<sup>10</sup> The Doctrine of a Future Life. N. Y., 1860.



savagery to the present, man's history has been one of disillusion and often disenchantment. His mind has been far more fertile in error than in truth. Few of his wants have been satisfied and surely no sage mind would feel secure in arguing from desire to attainment. The impetuous diathesis of the west may grow neurotic as it becomes free, rich and powerful, but it is all unavailing. Again, in the past it has made an immense difference, greater than we can conceive, whether or what races have heard about a future life, but many an illusion has been beneficent, and truth is a very different thing from utility. In view of it all and at this point perhaps more probably than at any other, one may abandon all hope or possibility of proof and base the hope of a future life upon revelation alone, or may go further yet like Bishop Courtney, who tried to refute all proofs of post-mortem existence, declaring that all men died body and soul and were extinguished, but that at some appointed time their spirits were revived and resurrected by the power of God. The other alternative is also familiar. "If our ship never reach port and if there be no haven it becomes us to keep all taut and bright, the sails set, and to maintain discipline." Virtue in and of itself is its own reward, and at the very most nothing else is asked even of the future life than an opportunity for the continuance of virtue.

Another bold attempt to argue from ideas to reality, to make a landfall by conjuring with the ideal content of the soul is Kant's famous argument, viz., reason always seeks the unconditioned. This is its very nature. Hence nothing less than the *summum bonum* will content it. This includes two things, perfection and happiness, the two great desires of all ages. The ancients thought each implied the other. The Hebrews believed that righteousness brought happiness in this world. The Stoics believed the highest joy was implicit in the nature of virtue, while the Epicureans, conversely, held that the highest happiness involved virtue. This does not suffice. The unity between the two must be not analytic, but synthetic and causal. In the very nature of things, each must bring about the other. In the sensible world of fact and experience, this is not so true as it is in the higher intelligible world. They must belong together, and the very conception of immortality is the greatest perfection joined with the greatest happiness. They must be united completely, and whereas in the phenomenal world of life their development and union are only partial, there must be an infinite progression to bring them into complete harmony, because a

being destined for perfection cannot be arrested. If this were not so, there would be no perfect virtue, and so we are immortal because this latter must be possible. On this view too heaven and hell arise and also fade together, so that if now the latter is becoming extinct the former will surely fail. True, the sense of justice, by which we judge life, drama, literature and novels, demands that the good always get their reward and the bad their punishment. This instinct is very deep and underlies law and society, but we have no warrant to say that the universe is built upon this principle. A serio-comic illustration of Kant is found in E. L. Master's "Spoon River Anthology" where the dead in the cemetery of that town sit up and speak, telling the true facts of their lives. One, e. g., says in substance, "My epitaph calls me good; it is a lie. In truth I did and said this, that or the other, so that all was vile." Another says, "They thought me an outcast because I did not hold to creeds I could not understand but in fact I lived for secret charity and exhausted myself working to serve the sick, poor, aged and sinners." Thus all in turn pass a confessional judgment day sentence or encomium upon themselves as if, like Plato's sage, they could not rest in peace without being known for what they really were. But neither the intellectual intuition nor even conscience, on the hedonic narcosis are constitutive principles. Moreover only the western world demands personal immortality, so that the conviction that no evil can befall a good, wise or truly ascetic soul, is only a sentiment or at best a postulate. Moreover what are truth, virtue, beauty, and how shall we define or even know them?

VII. A *seventh* group of views, distinct but with more or less affinity to each other, are those that entify the individual soul in the interests, more or less conscious, of immortality. Howison, e. g. ("The Limits of Evolution and Other Essays," New York, 1901, p. 396) makes pluralism absolute by advocating an eternal or metaphysical world of many minds, all self-directive, the items and order of experience of which constitute all real existence, even time and space. At least about everything else is logically implicit in their self-defining consciousness, the recognition of each by the other constituting the moral order. This makes an eternal republic or city of God, who is the "fulfilled type of their mind and the living bond of their union." They control the natural world, are sources of its law, and are free, for their essence is mutual relation. In this world of spirits God is

not solitary and there is room for the freedom of all. The joint movement that we call evolution is transient and can never enter the real world. Creation is not an event with a date but a metaphor. The key of everything is conscience and teleology. This view differs only from Leibnitz's monadology in denying grades and castes in these fulgurations of God. It makes objects in nature the manifestations of mental activity and therefore just as real as they, so the eternal reality of the individual is the supreme fact.

Royce too does not teach a psychology without a soul. Individualities are basal and teleological. They are aspects of the absolute life, therefore have a meaning. But in this present life, much as we strive to know and love individuals, there are no true individuals which our present minds can know or express. As we strive, therefore, to find real others, we realize that all that we know of them is but a system of hints of an individuality not now revealed to us, which cannot be represented by a consciousness that is made up of our own limited experience. Therefore the real individualities which we loyally seek to express get from the absolute viewpoint a final and conscious expression in a life which is conscious, the only life which idealism recognizes, and which in its meaning but not in time and space, is continuous with the fragmentary flickering existence wherein we now see so dimly our relations to God and eternal truth. ("The Conception of Immortality," also "The World and the Individual"). This argument, very dear to and very ably elaborated by its author, is obviously suggested by the Kantian postulate. Is it true in fact, however, that the closest companionship, friendship and even love, do not take us to the real individuality of the objects of these impulses? Though man has always been gregarious and social, it would seem that this instinct is abortive (if Royce is correct) and also that the reality of such an individuality as he postulates should not be conscious but either transconscious or frankly unconscious. He and Howison well supplement each other.

Miss Calkins in her various writings, although not consciously chiefly intent on proving immortality, belongs clearly in this group. The constant sense of self which she postulates in the teeth of the modern studies of multiple personality, harks back to Descartes, and she seems to the present writer to be a good illustration of Royce's persistent quest for a self that from its very nature can never be proven, a quest which in her has its chief strength, if analytically considered, in the personal satisfaction coming from the subconscious

reinforcement of reading and thinking in maturity as realizations of juvenile stages of development as the mind passed from the theological on to the metaphysical stage and beyond.

C. T. Stockwell ("The Evolution of Immortality"), assuming that all cells have an internal and an external or membraneous body, says that the Graafian follicle has a nucleus which reaches a state independent of its follicular body, and which we call an ovum, and has itself an external and an internal distinction of body. Being vitalized by the spermatozoon, it again develops externally as a placenta and internally as an embryo. The former dies when the embryo is born into the present stage of existence, and then we have the germ plasma over against the soma. Is it therefore not reasonable to suppose that our present external bodies possess nuclear bodies that in turn "evolute into forms suitable for external bodies, as we pass on one step more?" In other words, there may be something related to the germ plasma from which the individual sprang, as it is related to the rest of the body. This of course is only analogical reasoning and postulates a fact of which we have no scintilla of evidence. It suggests Shailer, who in his "Study of Life and Death" speaks of the molecular bridge that connects parent and child, which, small as it is, carries the germs of all traits of body and mind, so that in the present life we see the seeds of individualism carried on from one generation to another. "We may therefore see that the most complicated part of life is not that which goes out with the body death but that which is cradled in the infinitesimal molecule that is known to us as the germ of another life evolution." But this view, motivated by the lure of Weismannism, cannot go distinctly beyond it in assuming too great an absoluteness of the individual links in the sequence of heredity. Indeed the author's thinking is not so clear as to warrant discussion.

Edwin Arnold ("Death and Afterwards") simply assumes that the undeveloped cannot know the developed but can only presage it vaguely. He is platonic in assuming that life here is so beautiful that we may "rightly feel betrayed if dysentery or maggots end everything." Our fears may be as ridiculous as those of Don Quixote hanging from a window by the wrist over what he thought was an abyss, but when the thong was cut falling four inches. An authentic and transfiguring Yes, might be pronounced if we could recombine the chemical elements of the man analyzed in the South Kensington Museum into a vigorous youth.

Powell ("Our Heredity from God") thinks nothing can

destroy me but myself, that the flesh does not keep pace and is so left behind, and so escapes the terrible immortality doctrine with its dream of resurrection and judgment. The last utterances of life, which have often been clung to as so precious by survivors, are no more significant than the first cry of a new born babe. Immortality as generally held, he thinks, has produced more mischief than any false creed, for it has made this life seem tame and valueless. If doubt arises it has made man feel like a "plucked eagle shut up in a goosepen." Death is itself at the right time an object of supreme desire, and we should rejoice that God has put it in his program, and sing a love song to it like Walt Whitman.

An anonymous author asks, Why should the soul, the noblest and last goal of evolution, perish and the cosmos throw away its crown? It is the entelechy of all evolution. In general the best survive and only the worst become extinct. The great biologists has wrought from the beginning to give itself an organ to think through and mirror itself, and this momentum of self-preservation is too great to be absolutely arrested at death. So individuality must have absolute worth and be eternized because it is the key to and paragon of existence. It must be an *ens realissimum* because it has cost so much. Democracy too hypertrophies individuality. The Orient knew one was eternal; the middle ages knew that a few were; and the present is thinking that all are so. Our motto is "*Impariprogrediamur*" shouted with bravura. Self-conscious life is the highest of all possible categories, the model of all other unities by which they are understood, and not merely a symbol of ultimate reality but the thing itself.

Arguments of this sort are provincial. Man may be a mere microbe on our little dirt ball which the high gods could hardly see if the lentiform Milky Way were the object glass of a celestial microscope. What right have we to think that the cosmos accepts us at our own valuation? The great sphinx has for ages suckled children at its breast, only to destroy them with its claws. And when men die it reckes and cares not. As Fechner says, the plant world might say it was supreme and that insects, animals and man lived to manure its seeds. Vegetation preceded, nourishes and might at any time send out bacteria and miasma to clear the world of all animal life. Man is perhaps mean compared with the denizens of other worlds, and even his type, so precious compared to individuals, is worthless and may serve other ends. Despite his decadent but titanic pride and monumental ignorance of himself, and without his self nescience he would be

pessimistic. So tempting to the vengeance of the gods is his *hubris* or pride that to be disappointed about another life serves him right. The great saurians were once the highest creatures and seemed the pets of nature and the goal of all, but although their period was far longer than man's, they have passed. So perhaps the superman will sometime quarry and explore trace by trace the evidences of a human biped representing our own stage of existence, and modern man will be classified in a tongue yet unborn. Are we really nearer any ultimate goal than was the amphioxus? Man may thus be a link, which will sometime be missing, toward higher beings, more perhaps like Martians or Sirians, or denizens of the other twenty million visible stars. What right have we to assume anything so sacrosanct and fetchingly irresistible in the human type that the great Goodheart will never seek to evolve anything better, but accept us as a stereotype of finality? Such a supposition is pathetic in its naïve simplicity, and man as a race ought to rejoice that he can serve even infinitesimally some greater purpose.

S. D. McConnell revives a somewhat patristic idea that man is by nature mortal but is also immortal and can attain another life by piety and knowledge, as of old the Eucharist developed the potentiality of eternal life, or as the infant is a man, only dynamically. Man may become indestructible by a higher process of biogenesis. John Fiske said, "at some period in the evolution of humanity this divine spark may have acquired sufficient concentration and steadiness to survive the wreck of material form and endure forever." To be deified by righteousness would be a fit climax. This life is a period of probation or gestation in a new sense. Thus too hell is obsolete and the bad die, so that the great choice is between continuation and extinction. Those who desire a future life have the strongest motive to virtue, while others have only mundane motivations for good. Some crude preclusions of this were found among the Tauists who held that "the grosser elements of man's nature may be refined away and immortality attained even in this world." This could be done by an elixir of life, the desire to discover which, a century or more before Christ, became a national craze. So-called pills of immortality, taken in connection with certain rites and regimen, like alchemy which could make gold of baser metals, would purge away mortal elements and transfiguration and elevation might result, even for animals. (See e. g. H. A. Giles' "China and the Chinese," p. 143 et seq.). But where do we draw the line between the mortal and im-

mortal, for this may be as far above as the Tauists thought it was below us. May our posterity come naturally sometime to inherit what we now have to work out so painfully? Is man alone finishable? In fact, should not every stage of life, as we say of childhood, have its chief value for its own sake and be lived out fully, instead of being subordinated to a higher one? Surely the time has passed when we want an education that consists in prescribing just how the soul shall best moult the body. No doubt a moiety of our race to-day would hesitate before going in for a culture the chief end of which was to survive death. In many quarters it is now bad form to even discuss such questions because the world is becoming, in the phrase of Osler, Laodicean, indifferent or even agnostic, and leaves passionate affirmations of a future life, so in fashion in the days of Tennyson and Browning, to mystics or clerical rhapsodists, pectoralists or those steeped in cardiac emotions.<sup>11</sup>

VIII. An *eighth* and final group of views challenges the generality or the strength of the desire for another life. From a questionnaire of the Psychic Research Society it was found that very many did not feel it of urgent importance, did not wish to know for certain about it, and many did not desire it, although a few, like Huxley, would prefer hell "if the

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<sup>11</sup> Plato's proof is manifold. In the *Phaedrus* it is based on the spontaneity and power of self-motion of the soul. In the *Timaeus* it is because the soul is the *chef d'oeuvre* of the world, so wondrous and beautiful that the gods would not let it die. In the *Phaedrus* it is the struggle for knowledge which is the impulse to progress to ever more general ideas, so that the philosopher is simply in love with and practising death. Again, it must be immortal because no sin or evil can kill it. Again, the fact that all that live die must have a correlate or opposite, viz., that all the dead live, or as Cebes puts it, the latter is a necessary postulate to the idea of life. The soul too is simple and therefore undecomposable like ideas. Again, it is a harmony. The doctrine of reminiscence by which, *e. g.*, the slave boy Meno evolved the forty-seventh proposition of Euclid from his own mind, is perhaps to-day psychologically the most important, for when the idea of Karma or successive reincarnations, transmigrations and pre-existence was dropped, a support to the belief in and hope for a future no less important analytically than the doctrine of hell was lost. This trend expressed philosophically in Descartes' theory that animals were automata and now in the view that psychology has no domain outside consciousness, has had much to do in devitalizing the old type of belief, has made resurrection more like belief in the fourth dimension. The elimination of the above perspective, which has enabled a little group of psychologists, despite evolution, to keep each other in countenance in ignoring the unbroken genetic development of the soul, has hindered a greatly needed rehabilitation of belief in immortality.

conditions were not too rigorous," to annihilation. Metchnikoff has stressed this attitude. He finds in man's nature many and great discords and maladjustments with his environment. Many of his rudimentary organs reach their apex of development before birth; others decline in infancy and youth; some culminate in adolescence; others are in their prime in old age, and function hours after death. So man is a bundle of anachronisms, with organs and traits old and young, moribund and nascent, some relics or vestiges of the past and others buds. So man grows old and dies at whatever age with a majority of his faculties, while a minority demand more and longer life. In his relations to the world too there are discords. Instead of being a paragon, man is perhaps a fluke or sport of the anthropoid apes, preyed on by fears that his forbears knew not, and even at four-score cut off in the flower of his days, robbed of a score or two of years that ought to be his best, for most deaths are gruesome executions by microbes, or accidents, or hereditary handicaps. His soul is haunted by a submerged reminiscence of the immortality of his primeval unicellular ancestors, which divided forever, never died, and left no cadaverous elements. Man wishes to maximize himself, know, be, do, get all things; but in fact he must renounce most that he wants, school himself in resignation, make the best of defeat, and perhaps await nothing after this life. His sex nature is abnormal and both its indulgence and its restraint bring strain and worry and rarely is it successfully sublimated. Man is always attempting things beyond his power, not befitting his estate. He has built himself many a paradise here and yonder but all have faded. Schools hold the most opposite views on the supreme problems of man's destiny, purpose and good, which are really unknowable. Unlike the beasts, man has lost his hygienic and dietetic instinct or conscience, so that he has little sense of food or drink that is wholesome. He knows more than he can practise, so that there is a chasm between intellect and will. His conscience is often morbid and consciousness is not remedial as it should be. His life and mind are built on the foundations of a childhood far older, and instincts more sane and stable, so that his maturity is shot through with faults and flaws and what he calls his personality is little unified and liable to break up into constellations breaking out of their orbits under the stress of ancient impulses, so that his self-knowledge, self-reverence, self-control are only iridescent dreams. If we ask what should he do we can only say he may rejoice that at length he has



begun to know himself and to strip off his delusions about himself, and accept what science tells him. He must realize that his self-consciousness is a false god, fallible, partial, by no means the oracle he deemed it. It is a thing of shreds and patches, extemporized, accidental, transient, like the Experience he has so deified, and to deduce self-knowledge from the present moment is simply a very partial aspect of learning more about his unconscious reflexes and instincts, which have their being deep down below the threshold but which from their submerged depths rule his life. The only true knowledge of man is genetic, for the strongest elements of his being are precisely those that strike their roots deepest into the past and lowest down the scale of life. The true explanation of most that he does, thinks or feels can only be found in the immemorial antiquity of the race. What we call normal consciousness is not unlike the deposits of the recent quaternary age and we must penetrate below it, stratum by stratum; study every outcrop of the older formations, every denudation caused by disease, every psychic fossile of tics, obsessions, whims; use every anatomical clue, every hint from comparative psychology, disease, crime, and every rudimentary organ of body or soul, for we can find the only true knowledge of anything in the complete and accurate description of all its developmental stages. We must eliminate prejudice and especially beware of the very wide and subtle influences of the conceptions of an entified soul and a future life which make us so sharp for all that favors it and so dull to every negation. In fact, this lust for a future life has been so unconscious that it has pervaded and perverted man's whole endeavor to know himself. It would leave a vast void to evict all due to this bias, although of course we admit that curiosity about a future life has been a potent motive of investigation, which has prompted the assembling of many data which would not otherwise have been studied. We must nevertheless discard all scientific conclusions that are vitiated by the passion for immortality, and study man with new impartiality. In view of all this man's desire for a future life has been reduced to a very low ebb, for it is far less worth living for than our conceit of ourselves had supposed. But the fear of death and the forms of its mitigation are chiefly because man still dies so young. If we lived to an old age, not of Methuselic or even of Metschnikoffian span, and died symmetrically, not part by part, but with every organ and function ripe for death at the same time; if we knew senescence as fully as adolescence, we should find that gradually the lust for life

would be supplanted by an equally strong and counter-will to die. Perhaps we should come in the end to seek death actively, like the Wandering Jew or as we now seek life, going forth to meet it joyously as the supremest of all blessings. In such a case, we should have no immortality mania, for we should be satisfied with life here without desiring a sequel to it. The dreams of all forms of postmortem existence would be a nightmare. True macrobiotism means not only more years, but completeness of experience and absence of repression and limitation. Had we lived through the whole *comédie humaine*, drank all the draughts of bitter and sweet that ever were or could be brewed for man, we should feel toward all life under all conditions somewhat as we now feel when asked if we desire to go back and live our lives over again exactly as before in a kind of Nietzschean eternal recurrence. We need not shrivel up with age to a point and then vanish as we began before even the germinal cell from which we sprang was, but all the cells of our soma would drift by specialization so far away from the immortal germ plasm and also from each other that disintegration would be functional atavism with every bond of union lost. These of course are mere conjectures, but the fact remains that man is now cut off in his prime prematurely with much, perhaps most that he best thought is possible in life unrealized. Hence he is a pathetic creature doomed to a kind of Herodian slaughter, and he has always consciously felt this and so has ever cried out to the gods and to nature to have mercy. He has imagined answers to the heart-rending appeal which he shouted into the void and on this warrant he has supplemented this by another life which, when psychoanalysed in all its processes, means only that he has a sense that the human race is unfinished and that the best is yet to come. And so it is. Man's future on this earth is the real, only and gloriously sufficient fulfilment of this hope in the prolonged and enriched life of posterity here. The man of the future will realize all desires and live himself completely out so that nothing essentially human will be foreign to his own personal experience. The wish for a belief in immortality is thus at bottom the best of all possible augurs and pledges that man as he exists to-day is only the beginning of what he is to be and do, or the pigmoid or embryo of his true self. Thus, when he is completed and has finished all that is now only begun in him, heavens, hells, gods and discarnate ghosts, all will fade like dream fabrics or shadows before the rising sun. All doctrines of another life are thus but symbols and tropes in mythic terms of the

superman as he will be upon this earth. The great hope so many have lived and died in will be fulfilled, every jot and tittle of it, not in their own lives, but in the perfect man, whose heralds they really were without knowing it. Death bed visions will come true more gloriously than the dying thought. They died hungering for more life. The perfect man will die of satiety, passing over into aversion. The story will not be continued in a later number but completed in this.

Is there any true *thanatophilia*, the opposite of both thanatophobia and of the general love of life? Does the most complete and harmonious life bring not merely the quest for death, like that of Ahashuerus, as a way of escape, but an active striving toward Nirvana such as the Buddhists affirm? Will man ever come to love to observe the approach of death in himself and in others, just as we love to observe and study growth? Tortay, who lived to be 185, Parr who lived to be 152, Dretenerg who lived to be 146, the philosopher Renouvier who at 80 while reconciled philosophically to death dreaded it because, as he quaintly said, he had been so long in the habit of living, or the 150 centenarians who die in France each year, do not show it. If we live 200 years as Haller thought we should, or even 100 as Buffon and Flourens thought more fit, or if, as Irving Fisher thinks, hygiene has increased the average length of life among the prudent fifteen years, would the love of life tend to pass over to an ambivalent love of death? Of course the superannuated who feel themselves a burden or those in anguish do love it as a refuge, but this is not in point. Is not true euthanasia simply exhaustion and passive resignation or rather a simple surcease of the momentum of the will to live longer here or hereafter, simply because the lust to live has been satisfied to the saturation point? It is perhaps true that if all were cut off in their prime like Jesus another life would be vastly more desired and believed in. But a single case of ideally completed senescence terminated in a truly natural death, is yet to be demonstrated, and hence we lack final and scientific data by which to answer this question. Psychogenetically it is certain that the old lust for personal immortality has made man more anxious to prolong and enlarge this mundane life. The great and good things he expected beyond he now strives to attain here. He awaits more, not less, in this life because he expected so much of the other. Thus the old belief in immortality is one of the chief analytic roots of modern hygiene, although the question whether it can all go over into orthobiosis must remain open.

The study of *old age* (gerontology) seems destined to be no less important for both science and life than child study has proven. Also, as science is prolonging life, senescence looms up and must all the more be understood, and the revision of not only the scientific but popular views that have prevailed from Aristotle and Cicero down to Huefeland impends. Thanks to investigators like Minot, Jennings, Conklin and others, there is a very general, if vague, biological consensus which regards old age as a retardation of the rate of growth. The percentage rate of growth is greatest in infancy or rather in the very early foetal stages, and declines rapidly all the way to maturity. Thus decline in the rate of cell division is relatively greatest very early in life. Hence the younger the child or even the embryo, back almost to impregnation, the faster it is growing old. The child thus grows old by leaps and bounds while the old man grows old far more slowly. The most vital parts, too, keep on dividing forever, like the protozoa, hundreds of generations of which (twenty-five hundred by Woodruff) have been actually traced. In other more somatic and less germ-plasmal cells the divisions are reduced and changes more adapted to the environment. This specialization goes with death which has been slowly acquired and with very great advantage to the species, as Claparede thinks sleep was acquired, with a similar rejuvenative benefit. Thus every adjustment or every further step along lines of ontogenetic development, primarily serviceable to the individual, is gerontic. Vice versa also somewhat more special causes of age and death have been found, e. g. in the growing preponderance of katabolism over anabolism; in the accumulation of waste products which obstruct or poison; in the increase in size and function of the cell body relatively to the nucleus (as we see in fatigue, an adjustment which sleep rectifies); in the hyperactivity of phagocytes, whether as devouring higher and more vital elements or pressing against them; in Muhlmann's fatty granulation, which he found in various tissues; in deficient supply of blood and oxygen (as Nothnagel [*"Das Sterben,"* 1908] who observed himself clinically almost up to his death) held, believing that the ultimate cause of death was lack of blood or oxygen, i. e. smotheration or dessication; to calcification of arteries and other tissues; to malfunction of the thyroid, adrenals, sex or other glands; to the development and over function of the large intestine (in which chiefly Strassburger tells us twenty-eight million million bacteria, most of them wild and dangerous fauna, are produced daily); to strain, worry and anxiety, etc.

For most of these partial theories therapies, more or less definite, have been suggested, as if the old instinct that motivated a quest for an elixir of life or a fountain of youth still persisted. Hence we have Brown-Sequard's Testicular Extract, and preparations from other glands, Pohl's Spermine, Metchnikoff's Lactic Tablets, Gerochomy, the many cytotoxic serums in which the blood of animals is utilized to develop specifics against special diseases, the many chemical substances that experimental pharmacology is trying out, large doses of which kill while small ones have the power to stimulate specific functions, etc. Loeb and others have sought to show that instead of the normal stimulus to the ovum caused by the entrance of the spermatozoon which impels the whole growth process, chemicals applied to the ovum can cause the same processes of cell division to begin parthenogenetically, although they cannot bring the ontogenetic process to a finish or produce the mature adult. If old age ends in death as an unfertilized ovum does, we have thus a fresh suggestion as to how the former may sometime be prolonged if not rejuvenated by special treatment.

## BOOK REVIEWS

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*Sensory Discrimination in Normal and Feeble-minded Children.* By ANNA M. PETERSEN and E. A. DOLL. *The Training School Bulletin.* November and December, 1914.

This study is an attempt to discover if there are any significant differences between normal and feeble-minded individuals in the matter of pure sense discrimination. The field of lifted weights was chosen. The method of right and wrong cases was employed, with a standard stimulus of 80 grams and a graded comparison series from 82 to 100 grams. The upper threshold alone was determined; and this was defined as that intensity of comparison stimulus where a "right" judgment was obtained eight times out of ten. Equality judgments were eliminated by the instructions, or, if insisted upon, were considered as wrong judgments. Both time orders were employed. The results include liftings from 203 feeble-minded subjects, and as a control group 262 normal children were examined.

While working with the feeble-minded group, the authors discovered three types:—1. Those who absolutely failed to follow the instructions; these were almost entirely under six years mental age as measured by the Binet Scale. 2. Those who failed to make judgments in accordance with the instructions, *i.e.*, successive liftings with the same hand, but who could make judgments by means of liftings with both hands simultaneously; these were largely between 6 and 8 years Binet. 3. Those who were able to conform to the instructions; this group fell very largely over 8 years Binet.

For the feeble-minded group, the authors found a positive but low correlation between what they consider "discriminative capacity" and mental age, the  $r = +0.36$ . The average D. L. for this group was 12.6 grams for the second type, 7.8 grams for the third type. No correlation was found in this group between "discriminative capacity" and chronological age, the coefficients for the ages of 8, 9 and 10 years giving values of  $+0.17$ ,  $+0.19$  and  $-0.24$  respectively. No significant sex differences were noted for either the feeble-minded or normal groups.

For the normal group, the authors find a steady increase in average "discriminative capacity" with increase of age, the coefficient being  $+0.63$ . For this group they assume that the chronological age is the mental age as well. When we compare the curves of the "discriminative capacity" for each age for the two groups, we find that the curve for the normal group is lower except at one point (8 years). These differences are not great however, amounting to only one gram on the average.

From these results the authors conclude that:—1. "Increase in discriminative capacity is a function of intellectual rather than physical maturation." 2. Inasmuch as the differences between normals and feeble-minded (when we consider mental age) are so very small, and furthermore, since these slight differences may be accounted for in terms of attention and the like, this experiment does not have any

diagnostic value, except to ascertain whether the subject has the ability to conform to the instructions of experimentation. This latter is a diagnostic sign of 8 years Binet.

The problem attacked by Petersen and Doll is certainly an interesting one and is also one which might be of extreme practical value. Unfortunately, however, the authors have made certain errors of a technical methodological nature which, we believe, entirely invalidate their results. In the first place, their choice of a threshold value which gives 8 out of 10 correct judgments was most unfortunate. The recognized limen value is one upon which a greater or less judgment will occur with a probability of 0.5. The exclusion of the equality judgments by these experimenters, we believe to be absolutely unwarranted.

The authors, furthermore, speak throughout of "discriminative capacity," and this we believe they are unable to do on the basis of the results obtained. It will be remembered that they determined only the upper threshold and their "discriminative capacity" was the difference between this and their standard stimulus. Hence they assume that the point of subjective equality is the same as objective equality. Only on the basis of such an assumption could they obtain a measure of sensitivity of their subjects. It would be very curious indeed if the points of objective and subjective equality came out exactly equal or even approximately so—even though both time orders were employed. The authors fail to mention whether the rate of lifting and the time between liftings was controlled and kept constant, and this alone would change the point of subjective equality to a considerable extent. Furthermore, the authors admit that the instructions varied for different subjects to a considerable extent, and this again would have an effect. We must also disagree most strongly with Petersen and Doll's statement that there is no effect of practice in a lifted weight series. Such an effect is to be found and it again shifts the point of subjective equality. Hence, unless the authors know the position of this point,—and this they have no means of knowing from their present data—their determination of a measure of sensitivity is entirely invalidated.

Clark University.

SAMUEL W. FERNBERGER.

- I. *Suggestions for Revising, Extending and Supplementing the Binet Intelligence Tests.* II. *Psychological Principles Underlying the Binet-Simon Scale and Some Practical Considerations for Its Correct Use.* III. *The Significance of Intelligence Tests for Mental Hygiene.* By LEWIS M. TERMAN. Reprinted from the *Journal of Psycho-Asthenics*, Vol. XVIII, Nos. 1, 2, 3, 1913-1914. Pp. 20-33, 93-104, 119-127.

In this group of papers Terman presents a survey of the situation regarding many of the much-mooted questions concerning the Binet-Simon scale and allied attempts to measure intelligence. In I. he advocates the use of an entirely unselected group of children for standardizing any test, also the use of the round year number as the chronological age indicant, the adoption of the median accomplishment of a group as the norm for that age, and the shifting of certain questions in the present scale. He suggests that tests be developed which will arouse nearly the same factors in all subjects, which will be as free as possible from the personal equation of the examiner in their evaluation, and which will be real tests of intelligence and not of training. He suggests, also, the need of a standard pedagogi-

cal test of school achievement. II. discusses briefly the advantages of a test-series such as the Binet-Simon, the unjust criticism of it when it has been used to deduce facts which it was not devised to bring out, and its general reliability when used by a trained examiner. The need of skilful manipulation of the tests and the careful choice of the small amount of apparatus used is also discussed although Terman criticises the large element of chance in certain tests where that factor can be successfully eliminated by the use of experimentally controlled conditions. The usual methods of procedure are recapitulated with perhaps more than the usual emphasis upon the personal equation of the examiner. A less generally recognized result of the use of mental tests is presented in III. The Binet and other tests will in the near future detect and bring under social constraint many of the high grade defectives heretofore undetectable. Normal children will be freed from the handicap of contact with abnormals although it is doubtful whether such contact is as detrimental as the author declares. The child above the average will also profit, but here again the assertion that genius is often effectually starved and the mind prematurely arrested by too-easy school work remains to be proven. Nor is it likely, as the author predicts, that school promotions will be made on the basis of mental tests. A certain amount of knowledge is absolutely necessary as the basis for work in any grade and this must be determined by the pedagogical test of equipment and not by the psychological test of ability to acquire that equipment. Nevertheless our knowledge of the child in many phases will be furthered and made more valuable by what we may learn of him through mental examination.

FLORENCE MATEER.

Clark University.

*The Effects of School Life upon the Nutritive Processes, Health and the Composition of the Blood.* By LEWIS M. TERMAN. Reprinted from the Popular Science Monthly, March, 1914. Pp. 257-264.

This article presents briefly and succinctly the results of a number of recent investigations of the injurious effects of school life upon school children. School entrance itself is sufficient of a shock to cause retardation in growth and loss in weight. Nutrition and the chemical and functional properties of the blood are also affected. The various postures necessitated by certain school occupations are also efficient causes of poor breathing and scoliosis. In general, however, more investigations must be made before the importance of the individual factors can be determined accurately.

FLORENCE MATEER.

Clark University.

*Recent Literature on Juvenile Suicides.* By LEWIS M. TERMAN. Reprinted from the Journal of Abnormal Psychology, April-May, 1914. 7 pp.

A review of the prevalence of juvenile suicides in various countries with an inquiry into the probable cause and an estimation as to the condition in the United States. A larger number of such cases seems to be due to school factors than to any one other cause. Terman feels that it is the duty of the schools to work for the alleviation of the condition.

FLORENCE MATEER.

Clark University.



*The Circulation and Sleep.* By JOHN F. SHEPARD. University of Michigan Studies, Scientific Series, Vol. I. New York, The Macmillan Co., 1914. ix + 82 pp., with Atlas of 63 charts (82 figures).

From a study of the literature, Dr. Shepard believes that it is impossible to draw any definite conclusion as to an anaemia or hyperaemia during sleep. He studies various questions, then, making use of two trephined subjects,—one, an intelligent laborer, and the other, a senior at the University of Michigan. The book contains a valuable criticism of methods and a careful description of various appliances which Dr. Shepard had to devise or adapt in order to suit his purposes. In all, over 700 graphic records, amounting to 3,500 feet, were taken and this alone gives the study an authoritative basis. The brain volume, volume of the periphery, chest and abdominal breathing, blood pressure, heart rate, time of transmission of the pulse wave over the body, jugular pulse, and the size and form of the pulse were studied. "All of these were studied, not only singly and during ordinary sleep, but in combination and under various conditions designed to show the causes of the reactions found." Very great care was employed to eliminate from the records all extraneous influences,—the elimination of the influence of the movements of the scalp and voluntary muscles being especially noteworthy.

The results show that with the oncome of sleep, no matter how quickly nor what the position of the subject, the volume of the brain was markedly increased, the increase being from 0.3 to 0.7 cu. cm. There is also, in general, an increase in the size of the arterial pulse from the brain, accompanying the increase in volume with sleep. "Throughout the period of sleep, the volume of the brain remains higher than in the waking condition." Waking, on the other hand, brings a reverse of the circulatory changes,—volumetric and the like,—brought about by going to sleep. No definite correlation was found between the curve of the depth of sleep and the brain volume, except that the greatest volume occurs soon after the subject goes to sleep and as a rule disappears within 15 to 25 minutes. "During the greater part of sleep, the brain curve is at a level a little below this extreme." The results show that there is no definite reciprocal relation between the volume of the periphery (hand and foot) and that of the brain. With the oncoming of sleep, the breathing decreases in amplitude of the abdominal movements, with a relative increase in chest breathing. Various types of breathing were noted in sleep. There are also characteristic changes in the Traube-Hering wave during sleep. The blood pressure is 8-10 mm. lower during sleep than during the waking condition. In one subject, there was found to be a negative relation between the blood pressure and the rise and fall of the Traube-Hering wave; the opposite relations held for the other subject.

From a consideration of these results and many others which we cannot enumerate in this review, Dr. Shepard comes to the conclusion that there may be effective activity of cerebral vaso-motors under normal conditions. He is most emphatic on this point. "I do not see how we can avoid the conclusion that the brain vessels are not inactive, that they do not follow passively the changes in general arterial and venous pressure but that, on the contrary, they are under the quite definite control of a system of vaso-motor constrictors, and the center of this control is probably a portion of the general constrictor center in the medulla." "We reach the conclusion, then, that the brain

vessels relax on going to sleep and constrict on awakening with relatively great activity."

As a result of this conclusion, Dr. Shepard believes that we must definitively abandon an anaemia theory of sleep. Dr. Shepard notes other reasons for this:—1. there is an increase of brain volume to stimuli while the subject is awake and they certainly show no tendency to cause sleep; 2. there is definite evidence that the circulation change lags behind the mental processes. Hence the circulatory changes on going to sleep and on awakening should be looked upon as effect rather than cause. Dr. Shepard then advances, instead of an anaemia theory of sleep, one in which he considers sleep and sleeplessness as mental processes. "Sleep is promoted by the situation in which we have really become accustomed to sleep." "Sleep is controlled by conditions similar to those which control attention generally. Sleep and sleeplessness are mental processes." "As we go to sleep, then, we become absorbed in a mass or complex of fatigue sensations. These tend strongly to inhibit other processes, especially motor activity and consciousness of strain sensations in the muscles."

This study of Dr. Shepard's shows, in the first place, exceedingly great care in experimentation; and secondly, great conservatism in the interpretation of his records. The author seems definitely to have made his point of overthrowing the anaemia theories of sleep. His constructive work is, however, less convincing. His theory appears to be merely a carrying further of some of the auto-suggestion theories of sleep which have been in the literature for some years. However, whether one accepts the author's theoretical interpretations or not, one must grant that Dr. Shepard's book makes a valuable contribution to the literature of this topic. The charts are very well reproduced, very numerous and well selected and are of great help to the reader.

Clark University.

SAMUEL W. FERNBERGER.

## BOOK NOTES

*Das Interesse der Schulkinder an den Unterrichtsfächern.* Von GEORG BRANDELL. Leipzig, J. A. Barth, 1915. 168 p.

This is essentially a study of the feeling value of the topics of instruction, a report of which of them are loved and unloved, beginning with the lowest classes, comparing always results obtained by similar studies elsewhere. There is special reference to children's relative fondness for theoretical and practical departments. Children's judgments in regard to usefulness of different departments and the reason why they like or dislike them are interesting and significant. A special rubric is the desire of children to go farther in the different topics. The latter part of the report is devoted to fifteen special topics, one after another.

*The investigation of mind in animals.* By E. M. SMITH. Cambridge, University Press, 1915. 154 p.

This book is designed to be a primer or introduction to the subject, with a discussion of the modes of procedure, aim, trend, and the general nature of the results hitherto obtained. The writer confines himself chiefly to description and illustration, although incidentally drawing attention to certain difficulties. He begins with protozoan behavior, then takes up retentiveness, habit formation, associative memory, sensory discrimination, instinct, homing, imitation, evidence for intelligence and ideas. A brief and select bibliography is presented for each chapter.

*Pathological lying, accusation and swindling; a study in forensic psychology.* By WILLIAM HEALY and MARY TENNEY HEALY. Boston, Little, Brown & Co., 1915. 286 p.

This is the first of a series of monograph supplements to the Journal of Criminal Law and Criminology. The authors first give a very good account of previous studies and describe in over one hundred pages cases of pathological lying, swindling and accusation, then discuss them as border-line medical types. The strength of this book lies essentially in its description of cases rather than in its contributions in the line of making positive scientific conclusions. Although this may be disappointing to some, it is probably a merit and not a defect.

*Healing currents from the battery of life.* By WALTER DEVOE. Cleveland, Ohio, Vital Publishing Co. (c. 1915). 243 p.

The purpose of this book is frankly "to heal those who read and study its pages. The articles are gathered together with this object in view. Each one was written under the inspiration of a definite purpose to heal, encourage, enlighten. . . . The question is how much you incorporate of the positive thought of feeling and truth into your mind and body."

*Educational values and methods based on the principles of the training process.* By W. G. SLEIGHT. Oxford, Clarendon Press, 1915. 364 p.

The author first reviews experiments, discusses the theory of the common element, of concepts, of methods and ideals, principles concerning the selection of material, the curriculum, work, leisure, modifications of the course of study, and finally discusses the fundamentals of matter in teaching English and nature.

*Psychology of high-school subjects.* By CHARLES HUBBARD JUDD. Boston, Ginn & Co. (c. 1915). 515 p.

The author first discusses psychological problems in mathematics, which lead him to the psychology of space and geometry, number and abstraction, and on the basis of this proposes a reorganization of mathematics. After discussing the psychology of language in general he passes to English, foreign languages, the opposition between the practical arts and languages, and then on to mechanical skill as related to theoretical intelligence, and takes up industrial courses; then follow science, fine art, history, generalized experienced, teaching to study, and finally, the general problems of secondary education.

*Eugenics.* By EDGAR SCHUSTER. Baltimore, Warwick & York, n. d. 264 p.

This unpretentious little book contains eleven chapters, of which the chief are, eugenics in ancient times, Galton, its relations to evolution, Mendelism, statistical study of inheritance, inheritance of ability, defects, environment, selective agencies, social control.

*Psychology and parenthood.* By H. ADDINGTON BRUCE. New York, Dodd, Mead & Co., 1915. 293 p.

This is a general discussion of inheritance and environment, suggestion, the secret of genius, intensive child culture, problem of laziness, laughter, hysteria, and the menace of fear, with certain closing words.

*Essays towards a theory of knowledge.* By ALEXANDER PHILIP. London, George Routledge & Sons, 1915. 126 p.

The chapters are on time and periodicity, the origin of physical concepts, the two typical theories of knowledge, and the doctrine of energy.

*Fear and conventionality.* By ELSIE CLEWS PARSONS. New York, G. P. Putnam's Sons, 1914. 239 p.

This is a sprightly, popular book of seventeen chapters on such topics as not talking to strangers, travelers, hospitality, introductions, caste, presents, calling, entertaining, between the sexes, marriage, family and classes.

*The principles of evolution.* By JOSEPH McCABE. Baltimore, Warwick & York, n. d. 264 p.

*Significato bio-filosofico della guerra.* By WILLIAM MACKENZIE. Genova, A. F. Formiggini, 1915. 101 p.

*Ein experimenteller Beitrag zur Erforschung des Unterbewussten.* Von LILLIEN J. MARTIN. Leipzig, J. A. Barth, 1915. 164 p.

*Functional periodicity; an experimental study of the mental and motor abilities of women during menstruation.* By LETA STETTER HOLLINGWORTH. New York, Teachers College, Columbia University, 1914. 101 p.

*The recapitulation theory and human infancy.* By PERCY E. DAVIDSON. New York, Teachers College, Columbia University, 1914. 105 p.

#### PRIZE IN PSYCHOPHYSICS

Owing to the international composition of the judging committee, this Prize (see this JOURNAL, xxv., 1914, 148) cannot be awarded till the conclusion of the war.

E. B. TITCHENER

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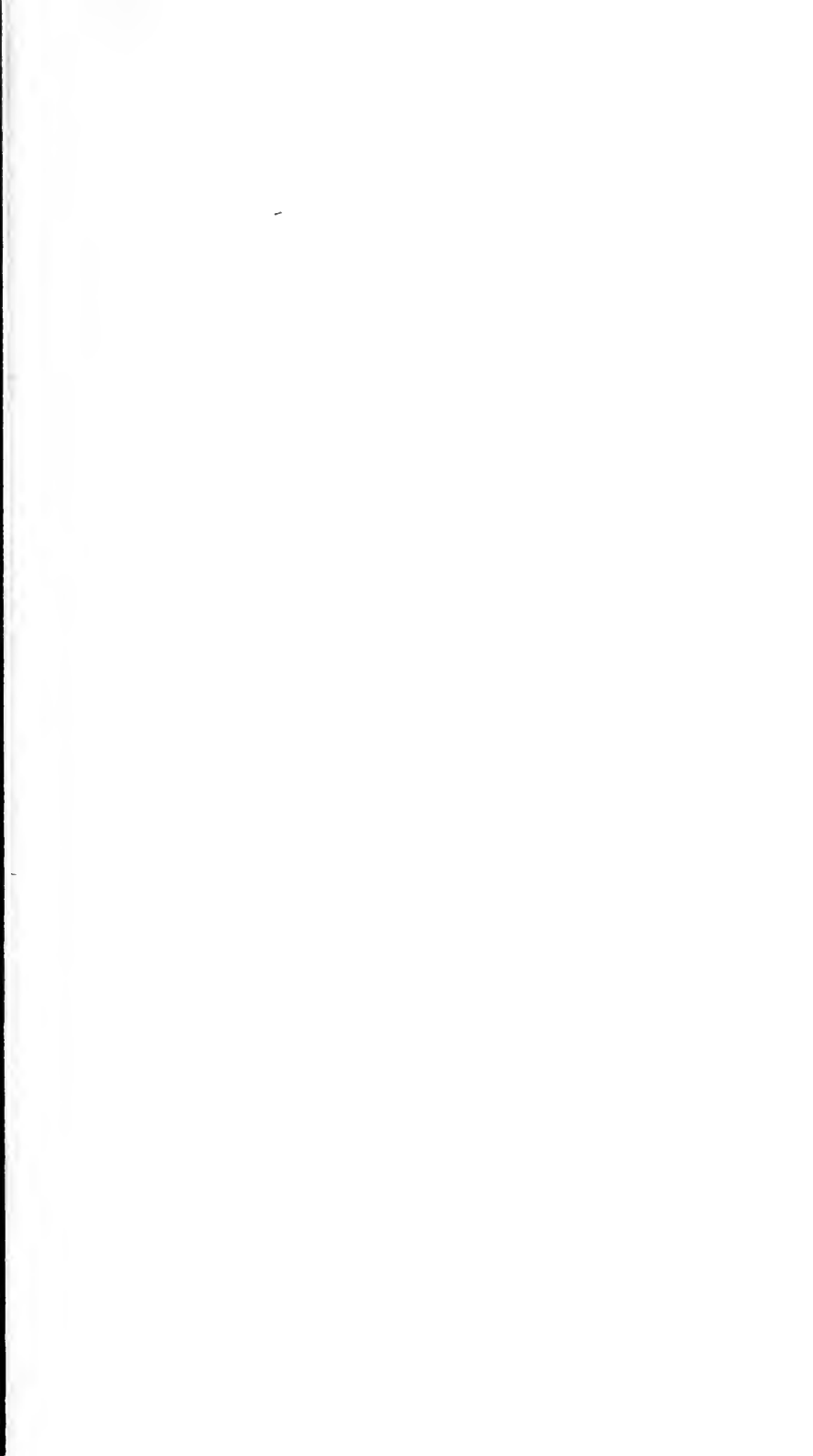
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